THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. Stanley Hall

Vol. XLI APRIL, 1929 No. 2

SEX DIFFERENCE IN THE ASSOCIATION OF IDEAS[1]

By Catharine Cox Miles and Lewis M. Terman, Stanford University

Bases of the Psychological Problem

Obvious differences between the sexes expressed in behavior and not readily traceable sociologically have been attributed to basic anatomical, physiological, and psychological origins. Differentiation between groups representing the sexes can usually be made on the basis even of fragmentary data in any of these three divisions of human development, and such differentiation may have a fair degree of accuracy. Even the individual members of sex-groups may generally be assigned to their proper classification by such means, though with a certain margin of error, which will usually be largest for the psychological, less great for the general physiological and anatomical features, but not insignificant even in the case of those indexes of sex development called sex characters, whether secondary or primary. Even in that one aspect of development, the endocrine, which lies at the base of sex structure and function, no complete dichotomy has been demonstrated; even here no absolute line of demarcation can be drawn.[2] Mixed or androgynous types may, it seems, appear even with respect to gonadal secretion, so that gonads of both sexes may be present in the same individual, or interstitial

*Accepted for publication October 20, 1928.
[1]Part of a study on sex-differences in non-intellectual mental traits, made possible through a grant from the Committee on Sex Research of the National Research Council.
[2]E. Sharpey-Schäfer, *The Endocrine Organs*, II, 1926, chaps. 53, 54, 55.

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

cells producing autocoids of one sex may be perversely produced in the organ characteristic of the other. Within human beings such extreme *Zwischenstufen* are rare, but they are known to exist, and they emphasize by their presence the range of distribution of sex characters in the human species, from the normal masculine to the normal feminine. Because the endocrine basis of sex has been demonstrated, a basis definitely prepotent during its period of activity, and because its effect upon structure and function in general has been demonstrated, there is a tendency to attribute sex differentiation in general to this origin. Furthermore, since a genuine and obviously fundamental difference has been demonstrated in this regard, and since other and secondary aspects of development appear to be conditioned by it, an inclination naturally arises (a) to look for further and often more remote differences that might prove as essentially diagnostic, whether as primary or secondary features, because of their being definitely the result of the demonstrated endocrine differentiators; and (b) to look for further and other differences as essentially diagnostic which may be due to some now obscure, but equally potent, differential causes.

Interest at present does not lie in demonstrating a superiority of one sex with corresponding inferiority of the other; for instance, current writers are not trying to prove an infantile type of woman or a primitive savage type of man, or again a feeblemindedness or mental weakness of woman and a brutal and tyrannical strength of man. Rather, the concern is with an evaluation of the available, experimentally demonstrated findings which may contribute to an understanding of present development under existing conditions,[3] and may also in part suggest the possibilities of future development under modified conditions. The problem is peculiarly complex because social conditions, the weight of tradition and the pillars of custom, maintain an equilibrium now that is not necessarily according to Nature's original construction plan. What we are able at best to ascertain is a series of sex differences as they appear under the present scheme of things; with perhaps some comprehension of the degree to

[3]H. C. Lehman and P. A. Witty, Play interests as evidence of sex differences in aesthetic appreciation, this JOURNAL, 40, 1928, 449-458; also J. A. McGeoch, The influence of sex and age upon the ability to report, *ibid.*, 40, 1928, 458-467.

which they are essential or fundamental as we see their deviations or fluctuations under more or less systematically modified conditions.

Some at present assume the existence of a masculine mind and a feminine mind,[4] and to this point of view the researches of Havelock Ellis[5] and others may appear to give support. "The masculine method of thought is massive and deliberate, while the feminine method is quick to perceive and nimble."[6] "Girls are aided by a better memory and a variety of emotional and moral qualities which involve a greater docility. Boys, however, possess other qualities which contain the promise of ultimately greater intellectual development."[7] "Men are better able to apply what they have learnt; men are very much more inclined to supplement what they have learnt by reflection or further independent investigation. . . . Women dislike the essentially intellectual process of analysis. . . . Women dislike rigid rules and principles and abstract propositions, they feel that they can do the right thing by impulse without needing to know the rule." "These characteristics, by which on the average women seem to be marked in relation to men, are *probably correlated with instinctive and emotional qualities which may fairly be regarded as organic*" (our italics). But again: "The facts are far too complex to enable us to rush hastily to a conclusion as to their significance. The facts, moreover, are so numerous that when we have ascertained the precise significance of some one fact, we cannot be sure that it is

[4]M. E. Haggerty and E. J. Kempf, Suppression and substitution as a factor in sex differences, this JOURNAL, 24, 1913, 414-425. "It must be said that the experimental study of adult sex differences has not as yet yielded any very significant results. There seems to be a general conviction that there is a feminine type of mind different from the masculine but in just what this difference consists we have as yet no measured experiments to prove." H. T. Moore, Further data concerning sex differences, *J. Abn. Psychol. & Soc. Psychol.*, 17, 1922, 210-214. "It is hard to escape the conviction that the original nature here depicted is of two fundamentally different sorts, and that the two types could hardly be permanently adapted to identical interests . . . There are very considerable and ineradicable differences in the original capacities of the two sexes for certain types of enthusiasm."

[5]Havelock Ellis, *Man and Woman*, 6th ed., 1926. Cf. also Joseph Jastrow, *Character and Temperament*, 1915 p. 366. "There is but one supreme natural differentiation—that of sex. If there is any distinctive group-psychology, it must be that of the group of men and the group of women. For men and women are organically different; which means that the physiological differentiation in the reproductive system involves a contrasted psychology."

[6]Ellis, *op. cit.*, 233.

[7]*Ibid.*, 261.

not contradicted by other facts. And *so many of the facts are modifiable under a changing environment that in the absence of experience we cannot pronounce definitely regarding the behavior of either the male or female organism under different conditions*" (our italics again). Briefly stated, the prevailing view of scientific investigators is perhaps this: a masculine and a feminine reaction-type may be differentiated and contrasted under existing conditions. The differences are perhaps correlated with organic factors, but are not inevitably so, for all of our data are gathered under such and such conditions and the influence of possible modifications of these is often, perhaps usually, doubtful or unknown.

Two problems are presented to the investigator in the field of sex differences: (a) to determine the facts of difference under the customary conditions of the present social order; and (b) to push back the boundaries of the obscure situations which offer variations from the present order and so, if only suggestively, to indicate the trends which educational or social modifications would or could bring about. The first problem has to do with determining the differences as they exist, the second with determining their essential or fundamental quality by tracing them to their origins.

In the field of analysis of sex differences, anthropology and sociology give consideration to large typical behavior differences indicating traditional and social modes. Psychology, however, is limited at present to detailed study of small behavior differences indicating as far as possible central tendencies in the individuals concerned. The former, the sociological and anthropological studies, are descriptive; they illuminate our own customs and prejudices and often show the social origins of our assumptions and beliefs. The latter are analytical and experimental, and they attempt to unravel the complexity of individual mental patterns and procedures that condition behavior. The association of ideas, with its implications for the building up of interests; memory, with its significance for learning; imagination, essential to invention, are examples of mental complexities subject to examination wherein discerned sex differences have practical as well as theoretical significance. The first of these is the subject of the present discussion and review. Methods of examination which investigate mental organization directly through introspection have given no positive, but rather a negative aid to our problem. Their analyses and conclusions are important for the study of the adult

normal mind,[8] and from them we incline to the dictum of Brissac that "Souls have no sex." It may be that more extensive studies of this character would indicate sex differences: the studies that have been carried on by equally trained observers of both sexes[9] give as yet no indication that this is to be anticipated.

Association of Ideas and Word Association Technic

The association of ideas can be investigated as a group problem, but for comparative purposes only indirectly through some standard, objectively observable, and measurable index. That in common use in the well known word association experiments and tests is the single verbal response, a symbol which serves only as a crude index to the pattern of the mental fabric in its rich complexity. A study of word associations is a study of established connections; it can view only indirectly the question of the original impression which formed the connection, of the interval during which it has become more or less definitely fixed, or of the attending conditions that have strengthened or weakened it. Only indirectly and circumstantially is it possible to trace the proportional elements of instinctive set-up or of habit impress which result in this particular word reproduction in response to that particular word presentation. So complex indeed is the whole matter that conclusive results for groups can be hoped for only in the presence of considerable numbers and where every effort has been made to achieve homogeneity and comparative equality of the groups compared.

Because of the compactness and apparent definiteness of the results achieved by word association technics, it has apparently seemed to investigators in the past, and at present also, that the mental behavior included under the general label of the association of ideas offers, through the medium of the verbal experiment, a field for investigating individual and group differences. Whether or not we account generally for the facts of mental life in terms of association, the arising of representations in consciousness in a certain relation to one another is a fundamental phenomenon of psychical activity. Parts of experience tend to reinstate other

[8] E. B. Titchener, *A Textbook of Psychology*, 1910, 374-428. Cf. also W. Wundt, *Grundzüge der physiologischen Psychologie*, 6th ed., 1911, III, 357-543.

[9] J. McK. Cattell and Sophie Bryant, Mental association investigated by experiment, *Mind*, 14, 1889, 230-250; M. W. Calkins, Association, *Psychol. Monog.*, 1, 1896, no. 2, 1-56.

parts, and perception, thought, and rational action are dependent on the functioning of the association process. It may be concerned with 'objects,' or 'ideas,' 'things thought of,' 'the thinking of them' or even 'mental attitudes' (complex undefined background). This is a matter of emphasis and of terminology; "so far as it stands for a cause it is between processes in the brain." We may say with James, association is "a result of the laws of habit in the nervous system," or with Titchener, "whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (of course in imaginal terms) all those sensory and imaginal processes which occurred together with it in any earlier conscious present."[10]

The difficulties in the way of mapping the course of integration or organization of stimulus material into the complex images of memory, imagination or generalization, in whatever mode, have been met and in part surmounted by psychologists whose aim was the observation and dissection of the individual observer's mental processes.[11] No study attempting group comparison has undertaken to sift the individual material for the comparison's sake. The task is clearly too formidable. Effort has been directed rather along the line of the verbal response technic, which since Galton's first study[12] has seemed to offer a kind of plummet for the ideational complexes that make up at least the upper layers of thought. The word association method plus introspection can go much further than this, but, as we have seen, the results are unwieldy for group study. In its simplest form the technic presents soundings of interests and mental occupations. For separate individuals, equally clear and regularly dependable samples of these activities are not obtainable, because (a) the soundings are from different levels, *i.e.* depending upon the degree of conscious or unconscious coöperation of the subject; (b) they are from regions in various degrees of neural tempest or calm, *i.e.* depending upon somatic conditions, fatigue, mental stress or relaxation, whether natural or induced; (c) they are variously predisposed by the character of the stimuli, so that the conditions of (a) or (b) or both may result in the case of subjects otherwise fairly equally

[10]Titchener, *op. cit.*, 378.

[11]H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens, *Arch. f. d. ges. Psychol.*, 4, 1905, 290-436.

[12]Francis Galton, Psychometric experiments, *Brain*, 2, 1879, 149-162.

balanced and motivated for the experiment. These confusing elements can in part be eliminated by the set of the experiment; the residuum of confusion can sometimes be stripped away. In certain instances these other factors are the very elements to be isolated and compared. In any case the experiment may be so set as to throw the emphasis either on the soundings, or on the accessory aspects, and the intrusion of other elements may be to a considerable degree diminished or eliminated.

"There are marked individual differences in association," says Titchener, apropos of his contrast between the quick and the slow learner. The associations of adults are complex, of children simple. The associations of the intelligent are rich, of the feeble-minded meager and threadbare. The associations of the normal balanced mind are clear cut, traceable, and when traced explicable; those of the abnormal mind are often distracted, inhibited or adverted by complexes, traceable only through intricacies of distorted meaning and reference, and in the last instance frequently inexplicable. Differences in the quantity and quality of associations may, in the case of the various groups, be obviously attributable to innate differences in the quality and strength of mind, or perhaps as obviously to training and experience. On the assumption that adequate analysis of mental associations through their symbolic expression may give a useful index to mental behavior and ability, intelligence tests and achievement tests are built. Controlled association material, and even certain free association technics, are depended upon as indexes of mental age or brightness. In the intelligence test scales no very significant sex differences in the handling of material of this sort have been found in tests constructed to measure mental development from age level to age level. In the tests of adults the case is not so clear, and in various association experiments of the free verbal type both qualitative and quantitative differences have been reported.

Classification of Word Association Data on Sex Difference

In bringing together the findings of psychological investigations of word associations where sex differences have been considered, arrangement of the material is suggested by content. The two main points of departure, in terms of the response and the responding subject, and in terms of the stimulus, might also be

derived from a historical review of the studies in their chronological order. The earliest study reported, Galton's introduction to the manner and method of verbal associations,[13] indicated the importance of the response as a biographical index, a plummet of personality, a means of revealing mental content. Féré[14] was interested in a somewhat similar aspect, viewed however from the psychiatric standpoint. The importance of this practical technic for investigating mental processes can not be overestimated. Its fault lay doubtless in ofttimes vague and subjective conclusions. An objective formal consideration of the effect of varying stimulus words was the natural point of attack of a systematic psychological investigation of the associative process by the word association method. Trautscholdt,[15] Cattell,[16] Sommer,[17] and Aschaffenburg[18] attempted to use common nouns or words without strong affective tone as stimuli, so that the results could be accurately and objectively classified in logical and grammatical categories. The ignoring of subjective or personality considerations in this approach was at once its virtue and its fault. It made possible some accurate measurements of certain mental procedures, but these were, as far as normal individuals were concerned, so abstracted from the engrossing concerns of life that their interest as indications of significant mental occupation was from this point of view slight. Apparently the most fruitful phase in the development of practical word association study came from a merging of the two emphases, and in general the more recent studies belong to various branches of this joint development: (a) psychological studies of mental age levels, sex, cultural, and occupational differences, and (b) psychiatric studies of mental deviations, normal and abnormal. Actuated by the interest of the inventors of the method in its dynamic significance as a cue to personality, contemporary investigators have combined with this interest a stand-

---

[13]Galton, *op. cit.*, 155, 158, 162.

[14]Ch. Féré, *La pathologie des émotions*, 1892, 328-343. "It is these personal variations that offer the greatest interest."

[15]Martin Trautscholdt, Experimentelle Untersuchungen über die Association der Vorstellungen, *Philos. Stud.*, 1, 1883, 213-250.

[16]J. McK. Cattell, Experiments on the association of ideas, *Mind*, 12, 1887, 68-74.

[17]R. Sommer, *Lehrbuch der psychopathologischen Untersuchungsmethoden*, 1899, 326-388.

[18]Gustav Aschaffenburg, Experimentelle Studien über Associationen, in Kraepelin's *Psychol. Arbeiten*, 1, 1896, 209-299; 2, 1899, 1-83; 4, 1904, 235-373.

ard of categorical objectivity introduced by the discriminative review of systematic psychologists.

For purposes of practical classification in a study of sex difference the two main approaches mentioned are probably sufficient: (I) in terms of the subject; and (II) in terms of the stimulus. The first may be further subdivided as follows: (*A*) Response type (level of intensity of attentive consciousness or degree of integration reached); (1) superficial, (2) coöperative and meaningful, and (3) guarded complex region, personal emotional level. (*B*) Personality type (emotional coloring of any level or degree of integration); (1) objective, logical, cold, and (2) subjective, predicate, or value type. (*C*) Interest type (racial, national, social, occupational, educational, age and other influences); (1) extent of interest, (2) character of interest, (3) value of interest, and (4) normality or abnormality of interest. (*D*) Form type (categories in various systematic classifications sometimes indirectly indicative of one of the three types given); (1) verbal, and (2) logical.

The second classification, in terms of the stimulus, may be more briefly considered in so far as sex differences are concerned. Subdivided, it includes (*A*) Modal forms of presentation; (*B*) Verbal forms of presentation; (*C*) Free and controlled association; (*D*) Influence of recording methods; (*E*) Influence of high tension words; (*F*) Influence of experimenter.

I. Classification in Terms of the Responding Individuals

(*A*) *Response type.* In individual studies the response type has usually been controlled by some explicit or implicit mental set or attitude. Trained *O*s, whether or not carrying on introspective analysis with a view to report, are usually giving their full attention and coöperation to a certain aim of the test. The meaningful response type results, in the main, unless (a) some distraction is offered of sufficient strength to push the responses to the upper or superficial level, or (b) the stimulus word is of such a nature, or the threshold of the individual in a particular case so close to the surface, that the push is downward and the region of emotional complexes is invaded. In group studies, where untrained individuals are usually the *S*s, the set or attitude is imperfectly defined, and may be described simply as the naïve response of a layman to a novel experiment. In an academic or school situation or where the task is a part of some requirement, a 'school set' may be assumed. Among non-academic *S*s, usually adults, the attitudes will be various. The size of the group must in general determine its representative character: if sufficiently large, variations from the usual will balance one another and the normal will

tend to predominate. Here, and otherwise as well, degree of understanding of the nature of psychological experimentation, practice in laboratory observation, and scientific training in general, will have their influence on response type. (It is assumed that controllable somatic conditions are maintained at a fairly constant level.)

Group studies of response type indicate typical difference trends for age, sex, cultural or educational level, and degree of mental normality. Each of these may be interpreted by comparison or in terms of the other, where each in part indicates the significance of the other comparison. A suggestion of the frequently more superficial response type of children, especially young children, is given by the Rosanoffs.[19] In this case the superficiality is due not so much to failure to coöperate in the adult sense, but rather to the imperfect attention of childhood plus (especially with the younger children) lack of familiarity with the words used as stimuli. In any attempt to explain adult responses as 'childish' both of these trends must be taken account of. The response types have been particularly observed in the normal discrete experiments of Jung and Riklin,[20] Fürst,[21] and Hans Huber,[22] and in the distraction experiments of Jung and Riklin. The latter have proved illuminating where tendencies in the former were too slight to be more than suggestive.

In the normal experiment the findings of the three investigations (whose chief emphasis is on other matter) present experimental facts on sex differences; and these in the main corroborate one another. The distraction experiment of Jung brings out more clearly the trends, noted in the normal experiment, that are produced by conscious or unconscious failure to coöperate with the intent of the experiment: trends important in reference to sex differences. Jung's normal experiment was one in which the *S*s, 22 women and 16 men, responded by giving orally the first word they thought of, to each of a series of 200 words presented as auditory stimuli. The reactions were timed with a one-fifth second stop-watch held by *E*, who also wrote down the responses. Two men *E*s (Drs. Jung and Riklin), whose reactions were found to be comparable, were in charge. The *S*s were selected with a view to their being average normal individuals, and probably they were justly rated as such. The 200 stimulus words were selected from a list of 400, which included 231 nouns, 69 adjectives, 82 verbs, and 18 adverbs and numerals. The composition of the

---

[19]I. R. Rosanoff and A. J. Rosanoff, A study of association in children, *Psychol. Rev.*, 20, 1913, 43-89.

[20]C. G. Jung and Fr. Riklin, The associations of normal subjects, in Jung's *Studies in Word-Association*, 1919, chap. 2, 8-173.

[21]Emma Fürst, Statistical investigations on word-associations and on familial agreement in reaction type among uneducated persons, *ibid.*, chap. 11, 407-446.

[22]Hans Huber, Ueber den Einfluss von optischem oder akustischem Reiz und grammatikalischer Form des Reizwortes auf den Assoziationsvorgang, *J. f. Psychol. u. Neurol.*, 23, 1918, 171-207. An error in the labelling of Huber's tables transposes all of the headings, apparently consistently. For J read AM; for AM read J throughout Huber's appended tables.

Jung list is too well known to require further description.[22] The words were selected as far as possible from those in common use. No attention was paid to the number of syllables.

The first distraction experiment made use of the third hundred of the 400 stimulus words. To these, reactions were made during a condition of inner distraction. The *Ss* (the educated group only) were requested to attend as far as possible to the 'A-Phenomenon,' *i.e.* the "sum of those psychological phenomena directly evoked by the perception of the acoustic stimulus." Directly after each reaction they described what they had noted during the experiment. The second distraction experiment used the fourth hundred of the word list, to which reaction was made under a condition of outer distraction. The competing stimulus was arranged as follows. To the beat of a metronome the *S* had to make pencil marks about 1 cm. in length. The beats for the first 50 reactions were 60 per min., for the second 50 reactions, 100 per min.

TABLE I

JUNG'S COMPARISON OF THE SEXES IN A NORMAL WORD ASSOCIATION EXPERIMENT*

(Results expressed in percentages)

Class of response	Men	Women
A. Inner Associations	38.6	40.7
B. Outer Associations	54.0	53.6
C. Clang Reactions	2.0	2.0
D. Residual group	3.9	2.4

*Abbreviated from Jung's table.

In the normal experiment Jung found, in comparing averages, minimal differences between the sexes. "With few exceptions the figures are in substantial agreement; at any rate, there is an absence of the distinct difference which, *e.g.* separates the group of the educated from the uneducated." In the distraction experiment larger differences appear, indicating in the main a lesser distractability of the women. The slight differences that did occur in the averages for the normal experiment are given in Table I. Calculations of the probable correctness of these differences between the entire groups of men and women (Table I) indicate, as Jung has stated, that they are not significant.

Considering averages for subgroups of *Ss*, Jung finds that both men and women of the educated group show smaller percentages of inner associations, and larger of clang reactions and residual responses, than the general averages for the combined groups. The uneducated show the opposite tendency. A possible difference in reaction type between men and women as groups is barely suggested. If present, it consists in the relation and balance of inner associations and the three other categories of response. Individuals who attend most seri-

[22]The complete list used by Jung and Riklin in this experiment is not given by them. Parts of it appear in Jung and Riklin, *op. cit.* 135 ff; 356 ff. In the translator's preface to the same work Eder gives a list of 100 words modified in form for use in English: *op. cit.* Transl's Preface vii. See also C. G. Jung, The association method, this JOURNAL, 21, 1910, 219 f.; and S. C. Kohs, The association method in its relation to the complex and complex indicators, *ibid.*, 25, 1914, 593.

ously and attentively to the object of the experiment, if they fully understand and if they give their best effort in order to carry out the instructions, are, Jung believes, apt to give a larger proportion of reactions associated with the stimuli in meaning. In so far as they do not give such reactions, their responses will fall outside of the inner reactions and in one of the three other classifications. The uneducated of both sexes give this more intent service to the test requirements, and in both cultural groups the women show the same tendency as compared with the men, the educated very slightly, the uneducated conspicuously.

The outer distraction experiment emphasizes the tendencies noted in the normal experiment, *i.e.* the women of both groups and the uneducated of whichever sex are less superficial in their response type, and although all groups are modified in the direction of superficiality by the distraction, the women and the less educated are less affected in this regard than the men or than the educated.

TABLE II

DIFFERENCES FOR THE SEX AND CULTURAL SUB-GROUPS IN JUNG AND RIKLIN'S 'NORMAL' WORD ASSOCIATION EXPERIMENT
(Inner Associations)

S group	No.	Average percent	Differences	Probable correctness of differences
Educated Men	9	35.2		
Educated Women	14	37.5	2.30	0.80
Uneducated Men	7	40.7		
Uneducated Women	8	48.7	8.0	0.98
Educated Men	9	35.2		
Uneducated Men	7	40.7	5.5	0.91
Educated Women	14	37.5		
Uneducated Women	8	48.7	11.2	1.00

Jung illustrates his points with averages only, and gives no measures of variability or reliability. The conclusions, based as they are on so few cases, seem even more striking and more significant in the light of further computations which we have made with Jung's data. In the normal experiment the average differences between the men and the women were notably small and insignificant for the sex groups as a whole. Selecting the inner associations, *i.e.* the most meaningful, the most indicative of the response trends noted, we have compared the averages by sex and cultural subgroups, and have computed the probable correctness of the small differences found. The results are given in Table II.

The cultural or educational status of the two sexes at each level should, however, be particularly noticed in view of the results. The educated women are rated as very intelligent in 3 cases, intelligent in 5, and fairly intelligent in 2; very well read, 2; well read, 5; comparatively well read, 3; cultured, 2; highly educated, 1; all-round culture, 1; poetical gifts, 1; university training, 2; have command of one or two languages besides the mother tongue, 2. The educated men are professional men with university training; 6 are physicians,

1 a medical student, 1 a science teacher, and 1 a chemist. Two points should be noted here to which Jung has not drawn attention, (a) the women are literary and linguistic in training, the men scientific; (b) no woman has, so far as mentioned, engaged in a professional or other public career, whereas all of the men are professionally occupied.

Most of the uneducated women were fairly intelligent, 5 had had schooling beyond the elementary grades, 7 were hospital nurses (in Switzerland unprofessional, a grade above the servant class), 1 a servant. The uneducated men were all fairly intelligent; all had attended the elementary school, one had further attended a technical school and one a secondary school, one was an intermediate school pupil, the others nurses. Some of the men of this group are rated as fairly intelligent; none of the women are so rated. Jung notes that the level of culture and intelligence is somewhat higher for the men than the women. The men were older, vocationally longer employed, more seasoned. The double contrast between cultural groups on the one hand and sex groups on the other is more significant when the above social background is kept in mind.

For the cultural subgroups, even the small differences are found significant (Table II), the probable correctness indexes being well above the 0.75 dividing line between significance and insignificance. Keeping in mind the cultural status of each of the four subgroups, we find that the averages follow these, from the educated men, whose cultural and occupational status is highest, to the uneducated women at the other end of the scale. The least significant difference is that between the two educated subgroups; the greatest is between the two groups of women at different cultural levels. The tabular results emphasize Jung's conclusion that sex differences are probably present, but also that cultural differences are far larger and more significant than they.

The inner distraction experiment was not given to the uneducated groups because of their inability to carry out its requirements. The small sex differences it reveals among the educated concern the personality rather than the reaction type.

The outer distraction experiment shows the largest differences between the sexes. These we have presented in two further ways, both of which serve only to emphasize Jung's conclusions. In the first place, computing the per cent of one sex scoring above the 50th percentile for the other gives some indication of the direction of the two distributions as compared with one another, see Table III.

The differences for the inner and outer associations are the largest sex differences found (Table IV), whether considered from the point of view of overlapping or of averages; they are also the most significant. The other two differences are probably significant in all but one case. Jung's conclusion that the men respond more superficially than the women and that the women are less affected than the men by distraction is 'probably correct' as far as all but one of the basal differences are concerned. A suggestion is made here as to a difference in training and experience which is typical of the sexes, but perhaps not therefore a 'sex difference.' Many women are accustomed to carry on mental activity expressed in reading or talking, at the same time that they are performing some manual task, such as knitting, sewing, etc. Hence, the marking of

strokes to the beat of a metronome while carrying on an association test at the same time may for some of them be less of a novelty than for many of the men. This experience would be quite as much that of the educated women as of the uneducated.

TABLE III

COMPARISON OF RANGE FOR THE TWO SEX GROUPS IN JUNG'S OUTER DISTRACTION EXPERIMENT

Compared Groups	First Experiment				Second Experiment			
	Inner	Outer	Clang	Residual	Inner	Outer	Clang	Residual
Per cent of men scoring above the 50th percentile of the women	23	69	54	54	15	77	23	95
Per cent of women scoring above the 50th percentile of the men	69	19	25	38	63	38	50	6

TABLE IV

PROBABLE CORRECTNESS OF THE DIFFERENCES BETWEEN THE AVERAGE PERCENTAGES FOR THE SEX GROUPS IN JUNG'S OUTER DISTRACTION EXPERIMENT

Kind of Association	First Experiment				Second Experiment			
	Men Ave.	Women Ave.	Diff.	P.C.	Men Ave.	Women Ave.	Diff.	P.C
Inner	29.9	37.2	7.3	0.96	25.8	32.9	7.1	0.96
Outer	57.5	51.5	−6.0	0.92	62.6	57.9	−4.7	0.84
Clang	6.0	6.7	0.7	0.61	4.1	6.4	2.3	0.81
Residual	6.0	4.7	−1.3	0.80	7.8	3.7	−4.1	0.99

Summarizing, we find from Jung's results, as far as response type is concerned, that in a normal word association test very small but probably significant differences between the sexes are present, and that in a distraction experiment the same differences are present, but to a more pronounced degree. The differences consist in a certain relation between inner (meaningful) and other (outer, clang and residual) reactions, and this appears to indicate the extent of deliberate coöperation rendered to the experiment. The women are more coöperative than the men in this respect, and show the tendency more pronouncedly the greater the distraction offered. But other subdivisions, namely, the cultural, show far greater differences than the sex groups in this regard. We may say then that women tend to be like the uneducated in their response type.

A summary of results for sex differences is incomplete without their interpretation in terms of the cultural, educational, or occupational status of the groups. Among the educated, where the difference between the response type of the sexes is least, the educational difference is also least. Among the uneducated, where the difference is greater, the difference in education is also distinctly greater and in the expected direction. Thus when the men show

a more superficial reaction type than the women, the difference cannot at once be assumed to be due to sex, as the same difference appears to a greater degree between the educated and the uneducated and appears to be measured by the cultural status. The educated "exhibit a distraction phenomenon as compared with the uneducated," and "we regard it as proved that the difference between the educated and uneducated reaction types . . . is entirely functional and has only the significance of a phenomenon of attention." As the same contrast in somewhat less degree appears between the sexes, and as the latter differ also in cultural level, the same phenomenon of attention and functional difference in type of reaction may be further concluded.

Had Jung based his conclusions on as painstaking a study of 500 *Ss* whose education and experience were similar or measurably dissimilar, in place of the 38 persons selected at random from two unequal social levels, his findings would doubtless offer the last word on the subject. But under the circumstances he brings us to the conclusion that the matter is still open to question and that the sex differences demonstrated are perhaps completely accounted for in terms of education and culture.

TABLE V

COMPARISON OF THE ASSOCIATION RESULTS OF FÜRST AND JUNG WHO USED THE SAME METHODS

(Table from Fürst)

Kind of Association	Men		Women	
	Fürst	Jung	Fürst	Jung
Inner	76.9	40.5	78.9	46.6
Outer	21.0	55.6	19.7	49.4
Clang	0.1	0.6	0.2	0.7
Residual	0.1	2.4	0.7	2.3

If we should interpret Jung's results in terms of children's reactions (Rosanoff),[24] the inattention of the men produces a response tendency similar to that of youngsters; the women on the other hand show more of that unfamilarity with the stimulus noted in the most youthful responses. However, we do not believe that the situations are more than superficially comparable, and we can not call either adult type 'childish' because of a slight factitious similarity in one aspect.

In a study of 42 men and 58 women drawn from 24 families, Fürst,[25] who follows Jung in method and treatment, notes the sex differences in response and gives the averages which illustrate them. As in Jung's study, the point stands out here "that education is one of the factors which most seriously influences the nature of the reactions." Fürst's *Ss* are from a lower cultural level and of less education and intellectual development than were Jung's. When her cases are compared with Jung's (Table V), very large and significant differences appear, although variations in method seem to be ruled out by the close collaboration of the two investigators. Such large differences may be connected with the large cultural differences present. The question of the suitability of

[24] I. R. Rosanoff and A. J. Rosanoff, *op. cit.*, 46-50, 88.
[25] Fürst, *op. cit.*, 407-446.

the classification method used is brought up by this large discrepancy in the averages. Perhaps, after all, objectivity is not obtainable, in which case findings in a single experiment may be compared but not experiment with experiment. Comparisons of the reactions of groups of men and women have indicated no such contrasting types for the sexes. However, Fürst finds some significant sex differences. She enumerates these from the point of view of the sexes as family members (fathers compared with mothers, brothers with sisters, etc.). Relatives of the same sex tend to resemble one another in response type, the women more than the men. The mean difference of related men is 4.1 (AD = 1.2); of related women, 3.8 (AD = 1.5). In the 6 families in which inner and outer associations were compared, the children under 16 were found to have more inner associations than the mother, and (with one exception) those over 16 had more outer associations than the mother. A similar relationship between the children and the father did not appear. In general, the husbands showed the masculine tendency noted by Jung and Riklin to react in a more superficial way than the wives, who here again demonstrated their more intense application to the required task. Exceptions appeared when the female *S* was more intelligent and better educated or better read.

Summing up Fürst's results, we find a situation different from Jung's as far as gross percents are concerned, but similar to Jung's in relations. The women tend again to give more meaningful associations than the men, wives than their husbands, daughters than their fathers; young children carrying the maternal tendency rather further than their mothers, older children beginning to become more superficial. The latter comparisons are based on too limited data to be more than suggestive, but the whole picture corroborates Jung's results.

Hans Huber,[26] in a study following Jung and Riklin, examined the reactions of normal and mentally disturbed individuals in a word association test. The material was presented in two forms, auditory and visual. Huber carefully discriminates between the responses of the two sexes, as well as between those of the other principal groupings of cases: normal, (educated, uneducated) and patients. The findings for the normal individuals, 17 women and 14 men, appear to be comparable to Jung's results. The word series used as stimuli were taken (a) from Jung's list, 200 words, and (b) from the lists of Aschaffenburg and Maier, 200 words. Half of each list was presented visually, the other half orally. The response was always oral. Jung's words are a heterogeneous collection characteristically of higher emotional tension than the other series, which is made up systematically of two-syllable common nouns.

The picture presented by Huber's figures (Table VI) is similar to that of Jung and Riklin, as far as the Jung words presented orally are concerned. The data give additional details through the visual-auditory comparison and through the Jung and Aschaffenburg-Maier comparison. We have computed averages for the normal cases from Huber's tables of the individual results (Table VI), and have derived indexes of probable correctness (Tables VII and VIII) for a few of the sex differences, selecting such of these as would be representative of the validity of the figures as a whole. The sex differences shown

[26] H. Huber, *op. cit.*, 171-207.

in the fourfold Jung classification are very small, and sometimes reversed: *i.e.* the men instead of the women seem if anything to tend to a more meaningful response. The difference for the Jung words is not significant. A rather striking difference occurs between Jung's data and Huber's in the percentage of responses classed as inner and outer associations. (Jung shows for inner associations less than 50%, Huber over 70%.) The Huber results for *S*s who appear from the description equal to Jung's uneducated are similar not to Jung's but to Fürst's results for less well educated persons (cf. Table V). The discrepancy brings up the question (a) as to the possibility of a sufficiently objective classification to make comparison valid, and (b) as to possible differences in the carrying out of the experimental procedure in various cases.

TABLE VI

SEX DIFFERENCES COMPUTED FROM HUBER'S DATA FOR NORMAL *S*s AND CLASSIFIED BY JUNG'S SCHEME

Kind of Association	Men				Women			
	Jung		A & M		Jung		A & M	
	Visual	Aud.	Visual	Aud.	Visual	Aud.	Visual	Aud.
	1	2	1	2	1	2	1	2
Inner	72.0	72.0	75.0	79.0	74.0	70.0	68.0	78.0
Outer	20.0	21.0	19.0	17.0	21.0	25.0	27.0	19.0
Clang	2.1	3.0	1.3	0.6	0.9	1.5	1.3	0.8
Residual	5.3	4.0	2.3	3.4	4.2	2.7	3.0	2.1

TABLE VII

THE RESULTS OF HUBER IN TERMS OF THE TWO GROUPS OF STIMULUS WORDS USED, WITH THE PROBABLE CORRECTNESS OF SEX DIFFERENCES IN INNER ASSOCIATIONS

S	Jung words	A & M words
Men	72.2 ± 1.94	76.8 ± 1.27
Women	71.5 ± 1.41	73.3 ± 1.44
Diff.	0.7 ± 2.46	3.5 ± 1.91
P.C. of D.	0.58	0.94

TABLE VIII

HUBER'S RESULTS SHOWING THE PROBABLE CORRECTNESS OF SEX DIFFERENCES IN OUTER ASSOCIATIONS AND CLANGS

S	Outer associations	Clang associations
Men	18.8 ± 1.49	2.75 ± .463
Women	22.1 ± 1.27	1.21 ± 1.93
Diff.	3.3 ± 1.95	1.54 ± 0.50
P.C. of D.	0.96	0.99

In Huber's results the sex difference for inner associations (using Jung's words) is not significant, but for inner associations with the Aschaffenburg and Maier words it is significant. For the outer associations (auditory presentation) and for clang associations, Table VIII, the sex differences are shown to be significant, and are in line with Jung's theory of the more superficial male response type. Taking into consideration all of the findings on the fourfold Jung classification, we are perhaps warranted in seeing in them neither confirmation nor disproof of Jung's theory. The *S*s in both of Huber's groups are so much more meaningful in their responses that the small sex differences are eliminated.

The light thrown on reaction type (a) by complexes (Table IX), and (b) by disturbances in memory (Table X), seems to add a small increment to the evidence for a consistent sex difference. The women in Huber's study reveal more complexes than the men, and they show less disturbance in recall; in other words, they are serious enough in general attitude so that emotionally colored stimuli break into the guarded complex region of personal experience in the original experiment; and they are also more successful in recall. These behavior indications, combined with longer time requirements, fewer clang and other superficial responses, especially a tendency away from the simpler grammatical associations to the more difficult and meaningful ones, appear to indicate that the group of women as in Jung's normal experiment is more subjective and coöperative than the men in fulfilling the stated requirements of the experiment. Huber adds no distraction experiment to emphasize this apparent tendency.

TABLE IX

HUBER'S RESULTS FOR COMPLEXES

		Jung words		A & M words	
S	No.	Visual	Auditory	Visual	Auditory
Men	14	7.1	8.1	4.3	6.4
Women	17	12.5	9.9	5.2	8.4

TABLE X

HUBER'S RESULTS FOR MEMORY DISTURBANCES IN WORD ASSOCIATION EXPERIMENTS

		Jung words		A & M words	
S	No.	Visual	Auditory	Visual	Auditory
Men	14	22.6	27.6	20.2	16.5
Women	17	20.1	27.7	15.2	14.2

We conclude that Huber's data for normal *S*s show small and probably insignificant sex differences in the normal experiment using Jung words. Complexes and disturbances in reproduction appear to substantiate the view that the women are more conscientious in response and that they keep closer, through meaningful reaction, to the hidden 'complex region,' and so tend to make attacks on their 'guard' more often than the men. It is not impossible that the sex of the *E* is entirely accountable for this phenomenon.

Hull and Lugoff,[27] for a list of 100 Jung stimulus words, find about the same number of complex indicators for the two sexes, 80 for the men, 76 for the women. Most of the words tend to indicate about equally for the sexes, but a few yield quite different values: *e.g.* for the women, *fall* and *choosing* are more diagnostic; for the men, *caring for*, *despise*, *pride*. The 100 *S*s averaged slightly over 66 complex signs each, their scores being 3,319 for the men and 3,320 for the women, indicating no difference in total number.

Differences were shown in kind of indicator, however, for of the four important indicators, (a) repetition of stimulus word, (b) assimilation of stimulus

[27] C. L. Hull and L. S. Lugoff, Complex signs in diagnostic free association, *J. Exper. Psychol.*, 4, 1921, 111-136.

word, (c) long reaction-time, (d) defective recall in repetition of reactions, the women show the larger number of signs in all categories except the last, *i.e.* in all except the memory test. In the four unimportant indicators, (a) no response, (b) repeated use of the same response word, (c) strange or senseless reactions, (d) obvious perseveration of idea, the women show a preponderance in the first two, the men in the last two.

The excess of the women in reaction-time the writers do not attribute, as do Wells, Jung and others, to the fact that *E* was a man, for if this were the case, they say, the women would for the same reason show an excess in the other complex signs as well, which does not occur. The women show an almost equal excess in the repeated use of the same response word and this is negatively correlated with long reaction-time. Conscientious coöperation is thought to be the cause for both long reaction-time and absence of defects in recall. Repetition of stimulus word also accounts frequently for long reaction-times in the women.

It should be especially noted that for three of the four signs which the authors believe to be really diagnostic, women yield the largest number of complex indicators. For two of the four unimportant indicators, they also yield the largest number. The remaining two total 0.67 in all, or less than 1% of all the signs, a very insignificant amount. Defective recall, the largest indicator for the men, does not occur in the original association experiment. It appears in a controlled association test in which memory rather than free association is the most important function. As the women have shown themselves more conscientiously coöperative from the first, we might expect more accurate performances in the reproduction test. Thus it appears that all of the 'complex signs' in the original experiment may be influenced by the sex of *E*, even if all do not correlate positively with one another. The matter is still open to question until the investigation is repeated under similar conditions by a woman *E*.

Burt and Moore[28] state that "the course of ideas is . . . more frequently disturbed in women by the various signs of complexes." It is their contention that "when words relating to specific emotions are distinguished and the times are calculated separately, the emotional influence and the sex differences are more prominent." They give us no specific data to support these statements.

Herrington and Willits's[29] study shows reaction-time as a measure of complexes in various categories, differentiating the sexes. Using men *E*s for the men, women for the women, they discovered more slow reactions among the women (48% compared with 40% for the men). The word list was highly selected, containing 30 words, mainly of high tension character for one sex or the other, or for both.

---

[28]C. Burt and R. C. Moore, The mental differences between the sexes, *J. Exper. Pedag.*, 1, 1912, 273-284, 355-389.

[29]L. P. Herrington and J. M. Willits, A study of sex differences in interest trends as indicated by the free association method. An unpublished experimental study made in 1927 under the direction of Professor P. R. Farnsworth, Stanford University.

Haggerty and Kempf[30] see in the women's less satisfactory responses in a controlled association test a greater number of conflicts than the men exhibit. Experimental procedure differences, unfamiliarity of material and differences in attitude were eliminated as possible causes for the difference. Even where the men had more conflicts, those of the women were more costly. Other possible explanations than a sex difference, *e.g.* imagery differences, have not been definitely eliminated, but do not seem sufficient as registered to account for the slow reaction-times found. A greater "tendency of the women to be on guard against embarrassment" is tentatively suggested.

Summarizing the experimental results with reference to response type in word association studies, we see that there is slight but fairly persistent evidence for a more superficial masculine response type compared with a more meaningful feminine one. Neither Jung's nor Fürst's results in their normal experiments would be adequate to prove the point, but combined with Jung's distraction findings they present a fair indication that a difference is probably present between the sex groups compared and under the conditions described. Huber's contribution to the response type differences is chiefly in his data on complexes; he finds women showing more of these. Hull and Lugoff emphasize from their extended study the more conscientious and coöperative response attitude of women. They add to this the conclusion that women tend to show more complex signs in the free association test than do men. The same conclusion, that women have more complexes as indicated in word association tests, has been reached by Burt and Moore, but they have not given their data. Herrington and Willits find more complexes among the women, as well as differences in type of complexes for the sexes. They used men *E*s for the men and women for the women. Haggerty and Kempf consider women more apt to be 'on guard,' indicating stronger complexes and perhaps more of them.

A further element in the evidence is that with reference to reaction-time. This matter has been treated at length in another place.[31] Suffice it to say here that the 'complex sign' of relatively more prolonged reaction-time is shown almost regularly by the women's groups. The only exceptions are cases where *E* has been a woman. In these an exact reversal of the situation has not, however, been demonstrated. Men are in general relatively quicker under the direction of a woman *E* than are women under the direction of a man.

This completes the picture of the more intense and meaningful coöperation of the women *S*s with the expressed aim of the experiment. It still leaves the matter open, so far as the evidence on sex differences as such is concerned. In the Jung experiment it appeared that the sex differences may be accounted for by known facts in regard to occupations, education and culture. Fürst's and Huber's cases offered no contradiction, and may in general be said to agree. Differences have in these cases been definitely demonstrated between these particular sex groups, but sex differences as such have not been shown. In regard to complexes the matter is less clear. Women showed the greater tendency to indicate complexes in the extensive study of Hull and Lugoff when the

[30]Haggerty and Kempf, *op. cit.*, 418 ff.

[31]C. C. Miles and L. M. Terman, Sex differences in word association reaction-time. Unpublished.

memory test was omitted, and it seems to us possible, for the reasons stated above, notwithstanding these writers' conclusions to the contrary, that this may have been conditioned by the sex of *E*. But Haggerty and Kempf point to such a condition where both the male and female *E*s were used. Here we are in a quandary. Can it be that the more attentive and conscientious effort of the women accounts for the situation in a controlled association test, and that it is simply a result of their less experience and consequently less self-reliance? If so, is this an innate sex difference, or a compensatory reaction in a world in which women have almost from the start less freedom for development and independent expression? These considerations lead us naturally to the question of a possible difference in personality type between the sexes.

(*B*) *Personality type.* The second grouping of studies of word association has to do with that more persistent mental attitude which is not a function of the test, but rather of the individual. It is exhibited in the test in the more or less subjective, or more or less objective responses which *S* tends to give as a general rule. Children's responses are somewhat more subjective than those of older persons (Fürst), but after maturity there is a second swinging of the pendulum to a senescent subjective type. It is only where the conditions can be varied and where, when they are varied, the response type regularly persists, that we can talk about a personality type. The principal contribution to this part of the sex comparison is therefore found in the Jung and Riklin study. These investigators differentiate a 'predicate' and a 'quasi-predicate' type and contrast them with an objective type. The true predicate type is applied to individuals who show a predominance of inner associations, especially predicates, not because of a particular attitude or set toward the experiment, but because of a persistent personality habit. These individuals "have, we assume primary vivid inner pictures," which make such a demand upon the attention that they persist even under conditions of distraction. The 'quasi-predicate' type unconsciously simulates the genuine type by a peculiar attitude (the uncultured) to the experiment. With individuals of this type the vividness of the stimulus image is due to the greater attention to the meaning of the stimulus which the uneducated, especially the uneducated women, show.

The data upon which Jung and Riklin base this conclusion are found in the preponderance of response in certain subdivisions of the fourfold classification already discussed. Where the inner associations prevail as compared with the verbal-motor forms, where the number of predicates averages about double the coördinate associations, where egocentric reactions and faults are registered to an unusual extent, a 'predicate type' is assumed. Vivid sense imagery, visual in character, prevents these individuals from becoming shallow or superficial or objective in their responses even when distraction is offered. For vivid imagery attracts and holds attention, and the personal, subjective set is unusually tenacious. The contrasted objective type responds especially with coördinates, coexistences, identities, verbal-motor forms, and clangs. It is easily distracted, becomes inattentive to the experiment, and its responses are then even more objective and superficial than before. Jung states that "from the figures of the distraction experiment . . . the predicate type is no merely

accidental momentary attitude, but corresponds to an important psychological characteristic—one which is maintained amid altered conditions." Jung says definitely that all women are not necessarily of the predicate type, neither are all men of the objective type. Many uneducated women persistently maintain their response type under distraction without showing the 'predicate' characteristics, while there are men of both groups who are definitely subjective. The educated women show relatively more true predicate tendencies than any other group. Here we are reminded (although Jung makes no mention of it) of the personal and value emphasis in their occupation and training and also of their literary (interpretative) rather than scientific (factual, experimental) training.

Among Fürst's *S*s the predicate type was present in both sexes, but relatively more women than men appeared to belong to this class. Relatively more men than women maintained an objective intellectual attitude. The predicate type seemed to increase in numbers with advancing age, but any noticeable change in this regard appeared later in life with men than with women.

In a table of comparison[22] between the sexes which is given as the basis for many of the conclusions on sex differences, we find the differences in the number of 'value predicates' (subjective type) and of 'definitions' (objective type) outstanding. It is these differences plus other slighter ones that lead Fürst to conclude that "the female subjects have a subjective 'personal' attitude whilst the men have generally an objective intellectual attitude." We have derived the PEs and the probable correctness of the difference for the value predicates computed from the individual tables given, and find them to be for the women, average 24.7 PE $\pm$ 3.2, for the men, average 13.6 PE $\pm$ 1.6. The difference is 11.1, and its probable correctness is 0.98, or practical certainty.

The original figures for 'definitions,' in which the men exceed the women 10 to 1 according to the table, are not available. Inspection suggests that these would show even higher probable correctness than the predicates.

A subjective personality type appears on the basis of these findings to be indicated more often among the women than among the men, and to be especially found among the cultured women. But Jung says that we may not assume it to be a sex difference because it is not common to women or absent from men. The best evidence for its existence is in the persistence of meaningful and subjective responses under distraction. But may we not attribute this, as we have said, in part at least to training and interest? Although there can be no question that a difference has been demonstrated for these sex groups, the question of its meaning will not be settled until comparable *S* groups are examined.

(*C*) *Interest type:* (*1*) *Extent of interest.* A series of studies of the community of ideas as evidenced by continuous word associations appears to show that men have larger surface vocabularies than women.

---

[22]Fürst, *op. cit.*, 442.

Jastrow,[33] Calkins,[34] and Manchester[35] have contributed results, the combined evidence of which is important on this problem. Each of the three experiments was carried out along similar lines; Calkins and Manchester having followed Jastrow's procedure. The *Ss* were all university students, first, 25 men and 25 women at the University of Wisconsin, (Jastrow, 1891); second, 25 women at Wellesley (Calkins, 1896); and third, 3 groups of 25 men each and 3 groups of 25 women each at the University of California (Manchester, 1905). The procedure consisted in asking the *Ss* each to write a list of 100 words (1) "at their leisure," and (2) "as rapidly as possible." The results gave information (a) on the average number (percent) of different words used by groups of 25 for each of the sexes, (b) on the number of unique words used by the groups, and (c) on the character of the words used. Conclusions to date on the first two points are based primarily upon these three researches. With reference to the third point, valuable results are available from other studies as well, and these we have attempted in later pages to bring into line as far as they concern sex differences. Figures from Manchester's table presenting the results for the three studies, to which we have added the percentages for the 'unique words,' give the various data in comparable form for both unique (unrepeated) and vocabulary (different) words.

TABLE XI

SEX DIFFERENCES IN COMMUNITY OF IDEAS AS INDICATED BY CONTINUOUS WORD ASSOCIATIONS

Investigation	Unique or Unrepeated Words*				Vocabulary or Different Words†			
	Number		Percent		Number		Percent	
	Men	Women	Men	Women	Men	Women	Men	Women
Wisconsin	746	520	29.8	20.8	1376	1123	55.0	44.9
Wellesley		868		34.7		1306		52.2
California Set I	1000	949	40.0	37.9	1471	1397	58.0	55.9
California Set II	1170	1079	46.8	43.2	1583	1509	63.3	60.4
California Set III	1079	978	43.2	39.1	1489	1407	59.6	56.3
Combined Calif. Set	1975	1950	26.3	26.1	3119	3048	41.6	40.6

*A word used but once and by but one person.

†Different words may be used more than once but each is counted as one only.

In both percentage of unique words and percentage of different words the men exceed the women in every pair of groups (see Table XI). The Wisconsin *Ss* show a large difference in number of unique words as compared with any other groups of 25. Manchester suggests that this may be due to different

[33] Joseph Jastrow, A statistical study of memory and association, *Educ. Rev.*, 2, 1891, 442; A study in mental statistics, *New Rev.*, 5, 1891, 559-569; Community and association of ideas; a statistical study, *Psychol. Rev.*, 1, 1894, 152-158; Community of ideas of men and women, *ibid.*, 3, 1896, 68-69.

[34] M. W. Calkins, Community of ideas of men and women, *Psychol. Rev.*, 3, 1896, 426-430. Cf. also Cordelia Nevers, Dr. Jastrow on community of ideas in men and women, *ibid.*, 2, 1895, 361-367; Amy Tanner, The community of ideas of men and women, *ibid.*, 3, 1896, 548-550.

[35] G. S. Manchester, Experiments on the unreflective ideas of men and women, *ibid.*, 2, 1895, 50-66.

methods of counting singulars and plurals: as the same, or as different words. In the California study they were counted as two different words. If Jastrow did not do this, it would account for his lower percentage of unique words. Another possible explanation is implied in Jastrow's statement, quoted by Calkins, that he would have rejected a list containing a long series of prepositions as too artificial and purely verbal. "What we want . . . is one hundred different ideas." Such a systematic weeding out of lists in a single group would of course give the resultant percentages a different significance from those obtained from unselected lists.

As noteworthy perhaps as the regularly larger percentages for the men is the decrease in the percentage differences between the sexes when groups of 25 are combined into larger groups (California combined set of 75). This suggests that the differences noted in the smaller groups, probably sufficiently reliable as measures in these particular cases, may not hold generally when other groups and especially large groups are examined.

In two other experiments, presumably with university students, Jastrow finds that when 10 common words are given one at a time, to each of which 5 responses are made, the number of repeated words, *i.e.* the community of ideas again, is (a) equal for the sexes (1891), and (b) larger for the men (1894). In a similar semi-discrete association experiment with high school boys and girls (1891) the girls were found to use distinctly fewer different words than the boys. In these three experiments the number of individuals is different from that in the studies first reported; the percentages are therefore not comparable with these and we do not give them. They are, however, comparable as between the sexes, experiment for experiment, and the conclusions are perhaps of interest as supplementary data.

Thompson,[36] Burt and Moore,[37] and Edwin Huber[38] in tests of a like character, *i.e.* semi-discrete word associations, oral stimulus, written response, found similarly that women tended to show rather less community of ideas than men. Thompson gave university students, 25 men and 25 women, a series of 10 words in response to each of which they were to write down as rapidly as possible for a fixed time, 1½ min., the train of ideas it started. The 10 words were selected from the common interests of university students. Of the 10 words, one called out more numerous associations from the men; for 4 the responses were equal, for 5 the associations from the women were more numerous. Combining results, it appeared that the men were more apt to stick to one topic, the women to cover a number and to have a greater number of associations. This agrees with Jastrow's results for discrete association series: *i.e.* when the stimulus word is given the women show less community of ideas (more variety) than the men. The results, however, are based on too few cases to be conclusive.

Burt and Moore report an experiment in which, proceeding from a given stimulus word, the *Ss* gave orally their associations, in a continuous train. The

---

[36]H. B. Thompson, *The Mental Traits of Sex*, 1903, 100-109.

[37]Burt and Moore, *op. cit.*, 382.

[38]Edwin Huber, Assoziationsversuche an Soldaten, *Zsch. f. Psychol.*, 59, 1911, 241-272.

women were found to alter their themes and topics far more frequently than the men, while the men showed a greater variety of associative connections between one idea and another with the same general theme. This seems to indicate results like those of Thompson but no figures are given to show the degree or significance of the differences summarized. Burt and Moore also report a second experiment in which responses in a discrete association test are examined. They give no data, but state that there are "conspicuous sex-differences" in the nature of the replies in children from five to eleven "even at the youngest stage."

Edwin Huber, using a list of 46 stimulus words, to which separate responses were given in writing, examines the word associations of 159 soldiers, aged 18 to 26, comparing them with Reinhold's[39] similarly obtained data from school-girls. The soldiers showed significantly fewer preferred reactions than the school-girls, *i.e.* they had less community of interest (more variety). Huber interprets this to mean that the soldiers were more guided by meaning than were the school-girls, and hence gave a more primitive type of response.

As the case stands, the evidence so far presented shows for continuous association a small but persistently greater uniqueness of response on the part of the males with greater community of ideas registered for the females. Where the groups are large the difference tends to disappear. In the discrete and semi-discrete experiments the situation is reversed; the women show less community of ideas, the men more. The matter is viewed from a somewhat different angle by *Es* who have attempted to correlate the extent of community of ideas with some other factor.

Certain experiments by German and American investigators have related the factor of age (especially mental age) to the diversity or community of ideas. Unfortunately for the present comparison, these were all carried on by the Jung type of discrete word association, in which a single response word is given to each of a series of stimulus words. Hence the findings are not comparable with those in the 'continuous series' of Jastrow, Calkins, and Manchester. They should be taken into account, however, in any study where sex differences are found in community of ideas, as possibly interpretative of such differences, and they may be compared with other results got by the discrete association technic. Meumann[40] concluded from studies of school children that "the richness in ideas and originality of reproductions, or the deviations of the reproductions of an individual from the average of the reproductions of children of the same age" (uniqueness) is a dependable measure of intelligence. Ziehen[41] found that children showed a greater number of individual associations than did adults. Wreschner's[42] results agree with Ziehen's, but he had only two children among his *Ss*, so his findings are no more than possibly corroborative. The tables of

[39]F. Reinhold, Beiträge zur Assoziationslehre auf Grund von Massenversuchen, *ibid.*, 54, 1910, 183-214.

[40]E. Meumann, Intelligenzprüfungen an Kindern der Volksschule, *Experimentelle Pädagogik*, 1, 1905, 91.

[41]Th. Ziehen, Die Ideenassoziation des Kindes, *Abh. a. d. Geb. päd. Psychol. u. Physiol.*, 1, 1898, 1-66.

[42]A. Wreschner, Die Reproduktion und Assoziation von Vorstellungen, *Zsch. f. Psychol.*, Erg. Bd., 3, 1907, 1-599.

Thumb and Marbe[43] and of Saling[44] are based on sufficiently large numbers to be significant in their results; they show that children diverge more than do adults from the common responses.

Reinhold proceeds to test the tendency suggested, namely to discover whether a continuous trend can be found from childhood to adulthood, where community of ideas increases (*i.e.* uniqueness decreases) more or less consistently with age. A group of 300 girls, ages 7 to 17, in 10 school grades were given a series of 46 words orally, writing the responses. The ratio of preferred reactions to all reactions averaged just more in the 5 upper classes than in the 5 lower, but the differences were quite small, and did not consistently increase from class to class. Furthermore, when the school-girls of 4 classes were divided class by class into more and less intelligent groups, no agreement was found between intelligence and variety of response. The question as to whether these findings contradict Meumann's depends upon whether the two experiments were comparable. In any case they fail to support the general validity of a conclusion that uniqueness and its opposite, community of ideas, in a discrete association test are measures of schooling or mental age. Similarly, the results with three groups of persons compared by Kent and Rosanoff[45] in their discrete word association test show both children under 16 and persons of common school education to have very slightly less than the average number of unique response words, while persons of college training have slightly more such words (the women of the highly educated group had just more unique reponses than the men). Kent and Rosanoff conclude that "this comparative study does not show any considerable differences corresponding to age or sex." Even the differences noted between cultural groups they believe to be the result of chance fluctuations such as might be reversed in another group of 1000 cases.

In conclusion we may say, with respect to community of ideas as indicated by free continuous association, that women and girls have probably a somewhat more limited field of expression than men and boys. In free discrete association tests the reverse seems to be the case. It is possible that time and place alter the situation, especially in the case of the free continuous association. We are inclined to the view that with more equal training and opportunity for experience of the sexes the situation shifts, and that this shifting may account for the Wisconsin-California differences. In the discrete association test, a matter of verbal facility as well as one of conscientious effort (such as occurs in the better school grades of girls) may enter in. Perhaps this is a fundamental sex-difference; perhaps again it has to do with training, opportunity, and experience, conditioned from the pre-school age onward.

(*2*) *Character of interest.* Extent of interest compared for the sexes shows small, possibly undependable, differences, conditioned perhaps by the social situation. Character of interest shows larger differences, but these are more clearly the result of social pressures and of habit.

---

[43]A. Thumb and K. Marbe, *Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung*, 1901, 17 ff.

[44]G. Saling, Assoziative Massenversuche, *Zsch. f. Psychol.*, 49, 1908, 421 ff.

[45]G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insanity*, 67, 1910, 36-96, 317-390.

The word association test obviously taps surface thought. It seems particularly adapted, when conducted as an unreflective response experiment, to bring to light habitual trends of mental occupation or lines of interest. It is quite probable that it does so except in two special instances, where the *S* fails to coöperate with the intent of the test, either (a) by reason of sophistication (Jung's cultured type), or (b) by reason of embarrassment (complex indication). The interference offered by these two possibilities is recognized as reacting possibly unequally for the sexes (a) because of the unequal sophistication of *S*s used, as far as reaction to this sort of experiment is concerned, and (b) because of the unequal emotional attitude induced in the two sex groups by reason of the investigator's belonging to one sex or the other.

In addition to the possibly fundamental difference referred to between results obtained for the two sexes there is, further, the formal difficulty inherent in any attempt at a classification of results. Classifications are subjective, and the results of one investigation classified by its investigator will never be strictly comparable with those that have been classified by another, even if he use with as much care as possible the same scheme of evaluation.

In the continuous association test the combined results of Jastrow, Calkins, and Manchester present a representative view of the sex differences in surface interests of university students in the 90's and in the first decade following. In the Wisconsin and California studies a group of men was in each case compared with an equal number of women. We may assume where there is a consistent preference of one sex appearing in all of these groups, or in 3 groups out of 4, for response in a given department of interest, that the difference is probably a real one. Manchester has presented these preferences in a table and we follow her, adding also the evidence of the Wellesley group. The men predominate in all 4 groups in (1) verbs, (2) implements and utensils, (3) occupations; they predominate in 3 out of 4 of the groups in (4) animal kingdom, (5) abstract terms, (6) parts of body, (7) other parts of speech, (8) miscellaneous, (9) meteorological and astronomical (10) mineral kingdom. Similarly, the women preponderate in all 4 groups in (1) wearing apparel and fabrics, (2) buildings and building materials, (3) interior furnishing, (4) educational, (5) arts, sciences, (6) amusements; and in three out of four in (7) adjectives, (8) conveyances, (9) kinship. Quoting Manchester, "In the three classes in which the men lead most pronouncedly, namely, in *verbs, implements and utensils,* and *occupations,* the notion of action is prominent. In the classes in which the women lead, such as *adjectives, wearing apparel and fabrics, interior furnishings* and *buildings and parts of buildings,* no such notion is evident. On the contrary these show a preference for things at rest." Further, the categories in which men lead show closer relation to time; those in which women lead, to space. Finally, Manchester touches on the most important trend running through both preference lists, namely, that each sex preponderates in the classifications that contain the objects and activities with which it is most concerned. The difference in the verbs used by the two groups brings this out conspicuously; men's verbs are concerned with their occupations and pursuits (examples are *shoot, tick, twitch, cure, hit, miss, aim, blow sail*), and women's with theirs (*address, telegraph, fall, rise, call, forget, ride, play, ringing, reading*). Without considering each of the classes in detail we may sum up the matter

as Manchester does, by noting that (a) the surface ideas of men and women are concerned with objects which are both familiar and interesting to them; (b) men are more inclined to interest in the dynamic, in the temporal, and in the abstract; women in the static, in the spatial, and in the concrete. It has been brought out and should be stressed again here that these two points are intimately related. The dynamic, temporal, and abstract aspects which the men's reactions present are surface appearances of men's habitual interests and concerns, as are the static, the spatial, and the concrete for the women. As Jastrow has pointed out, women's greater expressed interest in dress is natural because they must, and do, perforce spend more thought and time upon it than men do on their costume. The other differences shown are perhaps traceable to habitual preoccupation, and so we may regard them as sex differences in the sense that the sexes are occupationally divergent. If it could be demonstrated that the occupations of the sexes are psychologically conditioned as inherent mental resultants, and are not the secondary social resultants of the physical and physiological differences of sex, or that the differences persist even when the occupations do not, then we should have tangible sex differences as such.

If any group of individuals containing members of both sexes had been brought up without the typical traditional biases and pressures a comparison between their men and women might illuminate the question. At present we find the differences between men and women in word associations duplicated by those of certain occupational groups within the sexes, and so the question is still unsettled as to whether the sex differences shown are sex differences at all or the result of causes which training and custom may alter. Reinhold's school-girls and Huber's soldiers are profitably compared with one another in this respect. Of preferred reactions in discrete word association 44% were alike for girls and soldiers; of those not alike many given by the soldiers were obviously specifically related to the soldier's life; others were as obviously related to the interests of older persons (men) as compared with those of younger persons (girls); *e.g.* the girls respond to *play* with *ball*, the soldiers to *play* with *cards*. Another class of reaction words given by the soldiers was drawn from their life as working men before their entrance into the service. Huber found the effect of habit or occupation on word association further exemplified in the compared reactions of infantry, cavalry, raw recruits, and seasoned privates. Responses to a special list of words drawn from the common parlance of these various groups clearly indicated the influence of each type of military service and also length of service. Huber points out, as Wells has done in discussing the Kent-Rosanoff indexes, that word 'values' change with occupation and milieu, and this would seem an adequate explanation of the differences between the reactions of the school-girls and the soldiers. By implication this interpretation might be extended to differences between any sex groups where the occupations and habits of the two groups are, as here, dissimilar.

Wissler,[46] in reporting a group word association test given to Columbia and Barnard freshmen stated in his conclusions that the women gave 25% of parti-

[46]Clark Wissler, The correlation of mental and physical tests, *Psychol Monog.*, 3, 1901, no. 16, 1-62.

cular associations, the men but 6%; and he added that reading over the lists gave the impression that the women excelled the men in particular and personal associations. In kinds of association, no particular difference between the sexes appeared.

Burt and Moore[47] report that they repeated Jastrow's continuous word association test with children and adults, both English and foreign. "The results roughly confirm those of Jastrow; the females are more personal and subjective in their interests, the males more impersonal and objective." In a discrete word association test using a Jung or a Kent-Rosanoff list the differences in the nature of the replies are small for the younger children but 'most striking' for the older children and students. An example is the word *dear*, to which young children of both sexes give *love*, while older boys tend to give (1) *cheap*, (2) *mother*; girls, (1) *mother*, (2) *cheap*; men students, *fool*, women students, *me*!

TABLE XII

RESPONSES OF CHILDREN TO THE STIMULUS WORD *RED*
(Sample results from the Rosanoff data)

Girls

Age	4	5	6	6	6	7	7	8	8	8
Response	dress	goods	goods	yellow	paper	hat	desk	ribbon	dress	ribbon

Boys

Age	4	4	5	5	7	9	9	10	11	11
Response	dress	flag	line	flag	crayon	flag	iron	flag	chimney	color

Kent and Rosanoff[48] give no comparison of the sexes in the matter of word meaning in connection with their standard norms. The study by the Rosanoffs and Martin[49] of reactions to scientific terms belonging to the school curricula showed no sex differences in the value of words drawn from material to which the two sexes had supposedly been equally exposed. Selecting from the Rosanoffs' original data on children,[50] we find the first 10 girls and 10 boys responding to the word *red*, as indicated in Table XII. This example is representative of the trend toward sex differentiation in interest from a younger to an older age. It well illustrates the effect of habit and experience. Is there another factor involved belonging to what Jastrow once called, "woman's household instinct?" We are inclined to think that individual habits resulting from social requirements are again sufficient to account for the observed differences

Herrington and Willits,[51] examining 12 men and 12 women university students, found "appreciable sex differences in the intensity of interest and emotional color connected with words related to certain idea groups." The women

[47]Burt and Moore, *op. cit.*, 382.
[48]Kent and Rosanoff, *op. cit.*, 36-96; 317-90.
[49]A. J. Rosanoff, H. E. Martin, and I. R. Rosanoff, A higher scale of mental measurement and its application to cases of insanity, *Psychol. Monog.*, 25, 1917, no. 109, 1-113.
[50]Rosanoff, Martin, and Rosanoff, *op. cit.*, 54-71.
[51]Herrington and Willits, *op. cit.*, 60.

showed most interest in topics relating to personal appearance; men in topics relating to ascendancy (pugnacity, self-assertiveness, achievement). Both groups were equally interested in sex and in ethics.

Wyman includes in a report of her unique and significant *Tests of Intellectual, Social, and Activity Interests*[52] a summary of her results for sex differences. Her association test was made up of two groups of 60 words each given to 689 gifted children (IQs 140 and above) and 609 unselected children in grades four to eight, also to 156 unselected sixth and seventh grade pupils and 150 unselected seventh grade pupils. The words were selected as those most adapted to provoke responses due to intellectual interests and to social interests and activities. The lists may be described as composed entirely of (a) words in common use, (b) not too readily definable, (c) not too much tied with other words through continued association with them, (d) not words which would yield a large percentage of opposites in the responses, and (e) not words which would tend to produce carry-over effects. They were selected from a large list on the basis of actual results in a preliminary try-out. The age groups from ages 8 to 15 yrs. numbered for the gifted boys from 17 to 75 individuals, for the gifted girls from 15 to 64 individuals; the control boys (ages 10 to 15 yrs.), numbered 28 to 67 individuals per group; for the control girls (ages 10 to 15 yrs.), the age groups numbered 18 to 84 individuals. The ratings were based on qualitative valuations of the words, each word having three values established experimentally, one for intellectual, one for social, and one for activity interest.

The differences between the sexes are indicated by a comparison of averages and of variability. When in the case of all three interest categories the standard deviations are compared for the gifted boys and the gifted girls, we find that in 12 cases the measure is larger for the boys, in 12 cases for the girls. In the case of the control group the boys have a larger standard deviation in 10 of the cases, the girls in 7. With respect to the means, the gifted boys exceed the gifted girls in 13 cases; the gifted girls exceed the gifted boys in 11 cases. The means of the control boys exceed those of the control girls in 12 cases; the means of the control girls exceed those of the control boys in 6 cases. In no one of the categories and for neither of the measures (standard deviations and means) is the superiority or excess consistently in one direction. Apparently the differences in intellectual interests are not significant; those in social interest and in activity interest may be slightly so.

In contrast to all of the girls, both the gifted and the control boys show in these results a decrease in intellectual interests from 12 to 15 years. In control boys the curve of activity interest shows a moderate continuous rise, while with the control girls a fairly even level is maintained. In general Wyman's results indicate that "there are no sex differences in intellectual interest, but girls surpass boys at nearly all ages in social interest and boys surpass girls at most ages in activity interest."

The data assembled by investigators for the character of interest displayed by the sexes in word association experiments seem to show without exception

[52] J. B. Wyman, in Terman's *Genetic Studies of Genius*, I, 1925, chap. 16, 455-483.

that habit and occupation are important determining factors. College men and college women, school-girls and soldiers, persons of various ages, show characteristic trends. Men and boys tend to show more active and objective interests, women and girls more social and personal ones. We agree with Tanner[53] that "the experiments simply resolve themselves into a further illustration of the very well-known fact that habit determines the association of ideas," a fact that is equally attested by the different responses of persons of the same sex but of different professions.

(3) *Value of interest.* We have found that men have usually a somewhat wider field of mental occupation than women. Both sexes have been shown to follow habitual interests in their unreflective thought. The further question arises: does the vocabulary of one sex indicate a higher *value* of interest than the other? Are the word reactions of one sex more exact, more precise, more scientific, of better quality than those of the other? Does one sex give reactions of a higher grade than the other, word associations characteristic of better rather than of less well trained individuals? In order to test this matter as fairly and equally as possible the Rosanoffs and Martin[54] arranged a special series of 100 stimulus words drawn from the vocabulary of the science curricula of schools. They secured reactions from groups of individuals at each educational age from the fifth grade through college, adding also a group holding the Master's degree, a group holding the Doctor's degree (but not in the scientific directory), a group mentioned in American Men of Science but not starred, and one of the 'starred' individuals. All of the groups contained persons of both sexes. The reactions at each level were rated in four classes: (a) common reactions, a measure of the reactions having a value based on their appearance from average normal individuals; (b) high-standard value, a measure of the reactions having a value based on the responses of the 'starred' persons and the others listed in American Men of Science; (c) individuality index, a measure of the reactions not found in either of the foregoing; and (d) failure index, a measure of the failures to react. As to sex differences the authors found, on reviewing a table of comparison age by age from the fifth grade to the Doctor's degree, that when the sexes were rated separately on the total high-standard value, which they regard as "a more significant and more trustworthy measure of intellectual condition than any of the others made available by the test," the men made a very slightly better average showing in high-standard value in 9 and the women in 10 of the 19 groups; in other words no difference in this respect was apparent between the sexes. Furthermore the superiority of neither sex appeared more definitely at one part of the age scale than at another.

A difference was, however, noted in another regard: the men when they did not give a high-standard reaction were more apt to give a common reaction or an individual reaction; the women under these conditions were more apt to give no reaction at all. This situation was present at all ages and with all degrees

[53] Amy Tanner, The community of ideas of men and women, *Psychol. Rev.*, 3, 1896, 548-550.

[54] Rosanoff, Martin, and Rosanoff, *op. cit.*, 1-113; especially 88 f. and 95 f.

of education and selection. The authors say: "Perhaps this is a manifestation of the relative passivity, conservatism, lack of venturesomeness, and other such traits popularly attributed to the female sex."

The tendency to avoid failure of reaction by giving common or individual responses appears not only in men as compared with women, and in adults as compared with children, but also curiously enough in neuropaths as compared with normal individuals. The writers state, and we incline to agree with them, that the inhibiting mechanism acting in the case of the normal adults as compared with the abnormal is "distinct from the somewhat similar inhibiting mechanism existing in the female and which is lacking in the male sex."

Among Huber's soldier groups the old infantrymen showed more failures in reaction than did any of the other groups. These are the poorest material from the point of view of mental capacity. This tendency may be compared to that found by Rosanoff with children, and specifically with the consistent trend correlating with amount of dullness and school retardation among children of various ages. On the matter of response failures Freyd's[55] comparison of the responses of salesman and 'industries seniors' showed the latter omitting more responses than the former. The industries seniors were also more apt to be subjective, personal, and meaningful, *i.e.* more like the uncultured and feminine response types.

With reference to value of interest, the significant findings so far indicate no difference between the superficially available knowledge of groups of equal training when plumbed in the particular departments of equality. The sex difference appears in a secondary part of the reaction, namely, in the gaps between adequate knowledge; these are filled in by men with less satisfactory responses; by women with no response at all. The masculine fill-in is characteristic of the superficial, intelligent, objective type; the feminine of the meaningful, inexperienced, subjective type.

(4) *Normality of interest.* Normality of interest has been related to community of response by the discrete free word association method. Jastrow found that men tended to show slightly more community of interest than women on a semi-discrete test. Kent and Rosanoff[56] in a much more extended study make possible a wider comparison of sex groups. By the use of stimulus words of the non-personal sort selected by Sommer[57] from everyday experience, responses were obtained for both normal and abnormal individuals. When normal individuals, rated each as to the uniqueness of his responses, are compared in sex groups no appreciable or significant differences are found. Groups at three levels are contrasted; (a) school children, (b) persons of common school education, and (c) persons of collegiate education. The educated gave just more unique reactions than the other groups, the latter showing practically no deviation from one another. The very slight and probably in-

---

[55]M. Freyd, The personalities of the socially and mechanically inclined, *Psychol. Monog.*, 33, 1924, no. 151, 1-109.

[56]Kent and Rosanoff, *op. cit.*, 36-96; 317-390.

[57]R. Sommer, *Lehrbuch der psychopathologischen Untersuchungsmethoden*, 1899, 326-388.

significant group average differences show women at college level with more, and women at the common school level with fewer unique responses. These differences the authors say might disappear in another thousand cases.

Abnormal individuals of both sexes show up to four times as many individual reactions as do the normal. McClatchy[58] correlated community of response with various tests of intelligence and found that, in general, conformity to the norm or 'community of ideas' is indicative of normality, and that "the most normal person(in this sense) is the most generally intelligent." The normal individuals are those whose responses are in fact most superficial, but this superficiality "arises not from flightiness of ideas or unsteadiness of mind, but rather from a profundity of ideas and higher type of intellectuality."

The Casons[59] compared the distribution of response frequencies for 14 men and 36 women tested by a male *E* using the Kent-Rosanoff series, as against those for 20 men and 30 women tested on the same words by a female *E*. The average results for distribution of response showed no appreciable sex differences. Individuality or uniqueness of response was considerably greater for the females with the male *E*, and for the males with the female *E*. The chances out of 100 that the sex of the *E* will affect the result in the direction observed were 88, a fairly reliable indication. It was also to be expected that longer reaction-times would appear with the greater uniqueness of response. This again agrees with McClatchy's findings. Freyd's results show that community of response appears in his extraverted salesmen oftener than with his more introverted students. This would agree with the very slight difference between Kent and Rosanoff's[60] academic (presumably more introverted) and non-academic (presumably more extraverted) groups.

In their study of children, based on 300 cases, 25 at each age level, the Rosanoffs[61] found a fairly gradual increase with age from 4 to 14 in the number of common reactions, and a fairly gradual decrease with age in individual reactions and in failures of reaction. The increase in the number of common reactions seemed to correlate with increasing familiarity with the stimulus words, *i.e.* an increase in usable vocabulary. The decrease in individual reactions apparently occurred progressively as the children became mentally mature enough to mobilize attention and direct it quickly to a new stimulus word. A positive correlation was found to exist between community of association and mental capacity as rated by teachers on a 3-point scale: bright, fair, dull. Also between association and school grade a positive correlation was found which was doubtless in part due to mental capacity, but perhaps also in some extent to amount of education.

Concluding with reference to normality of interest, we find that this measure of mental maturity, adulthood, and general intelligence shows no appreciable sex difference. Small fluctuations in particular occupational or other groups are influenced perhaps by introvert or extravert characteristics.

---

[58]V. R. McClatchy, A psychological study of linguistic abilities with reference to the results of word association tests, *J. Exper. Psychol.*, 7, 1924, 371-382.

[59]H. Cason and E. B. Cason, Association tendencies and learning ability, *J. Exper. Psychol.*, 8, 1925, 167-189.

[60]Kent and Rosanoff, *op. cit.*, 45, 400.

[61]Rosanoff and Rosanoff, *op. cit.*, 43-89.

(*D*) *Form Type:* (*1*) *Verbal.* In the normal discrete word association experiment Jung found little difference between the sexes in the form of the response word. Women used just more words of the same grammatical form as the stimulus, and perhaps more words of the same number of syllables. Men used more alliteration, more consonance, and more words with the same ending. With a few exceptions the same scheme of slight differences persists for all cultural groups and all types of experiment used by Jung. Under distraction the women tended to have fewer responses with the same number of syllables, the men somewhat more. Hans Huber found that the men used rather more nouns and adjectives in response, the women more verbs, adverbs and pronouns. Jastrow's and Manchester's male *Ss* gave definitely more verbs than did their female *Ss*. The Wisconsin (Jastrow) men exceeded the women in number of adjectives, but for Manchester's *Ss* the reverse was true. In "other parts of speech" the Wisconsin men and two of three groups of California men exceeded the women. Edwin Huber's soldiers were more apt to prefer (a) adjectives, (b) verbs; Reinhold's school-girls showed the reverse tendency.

The data are corroborative of Jung's theory of the more and less meaningful types. The better educated, usually the men, tended to respond with the easier word forms, nouns, adjectives; they tended also to a certain superficiality as far as form was concerned, giving words of the same or similar sound rather than following the grammatical form. The Jung, H. Huber, E. Huber and Reinhold results seem to agree with this interpretation. The Jastrow and Manchester data do not necessarily agree, but they offer no obvious contradiction.

(*2*) *Logical.* Jastrow,[62] in the semi-discrete word association, found masculine preferences for associations by sound, from whole to part, from object to activity, from activity to object, and those by natural kind. Feminine preferences were for associations from part to whole, from object to quality, quality to object, and miscellaneous. In 1891 the same investigator[63] had found at least a strong suggestion that men favor associations by sound and from part to whole, women from whole to part and from object to quality. None of the differences is very large and all of them are dependent on subjective classification: at least in one case the results in the two investigations are apparently contradictory. Cattell and Bryant found (1889)[64] that four educated adults who teach and write favor logical and verbal associations; students favor the category whole to part; English school-girls give less than half logical and very few verbal associations; Irish school-girls give more than half logical and 8% verbal responses, perhaps as a result of their more literary training; London school-girls give about 1% verbal associations; their two teachers give respectively 33 and 26%. The two educated male adults engaged in abstract studies give three-quarters of their responses in the logical class.

[62] Jastrow, Community and association of ideas; a statistical study, *Psychol. Rev.*, 1, 1894, 152-158.

[63] Jastrow, A statistical study of memory and association, *Educ. Rev.*, 2, 1891, 442 ff.

[64] Cattell and Bryant, Mental association investigated by experiment, *Mind*, 14, 1889, 230-250.

As Cattell and Bryant point out, their, and any similar, classification simply makes subdivision convenient; it does not necessarily indicate "natural kinds." The groups run into each other and reclassification of the same responses would not agree exactly with the figures obtained the first time. They lay no stress on the sex differences in their data. Jastrow, on the other hand, sees in the differences indicated in his study such as "may be brought into relations with recognized differences in the mental processes of men and women."

II. Classification in Terms of the Stimulus

In terms of the stimulus, *i.e.* with varying types of presentation, a few studies have brought out what have been described as sex differences in response.

TABLE XIII

Sex Differences in Terms of Percent on the Basis of Mode of Stimulus
Computed from the original data presented by H. Huber for normal *Ss*

Response group	Men		Women	
	Visual	Auditory	Visual	Auditory
Inner	73.0	75.0	71.0	74.0
Outer	19.5	19.0	24.0	22.0
Clang	1.7	1.8	1.1	1.2
Residual	3.8	3.7	3.6	2.4
Egocentric	1.7	1.3	1.6	0.8
Time in sec.	2.4	2.2	2.8	2.7
Complexes	5.7	7.2	8.3	9.1
Disturbances in reprod.	21.2	22.0	17.6	20.9

(*A*) *Modal forms of presentation.* Dooley[65] and Hans Huber[66] have made comparisons of auditory versus visual presentation. Dooley's investigation made use of two different methods of (time) measurement for the two modes of presentation, so that her results are not strictly comparable for our purpose. Huber's investigation presents data for the two sexes which we have averaged as far as the normal *Ss* are concerned, and present in Table XIII. The figures show that for both sexes there are more inner and clang responses with an auditory stimulus; more outer, residual and egocentric responses with a visual stimulus. In terms of our earlier discussion it would seem that the more meaningful and coöperative responses are obtained with the auditory stimulus. The tendency is barely more pronounced with men than with women, which is apparently contrary to Jung's suggestion. The results with respect to time are also contrary to Jung's findings, for the more meaningful (auditory) responses are the faster.

The educated of both sexes tend in Huber's experiment to react more rapidly to the visual than to the oral stimulus, but the reverse appears to be the case

[65]L. Dooley, A study in correlation of normal complexes by means of the association method, this Journal, 27, 1916, 119-151.

[66]H. Huber, Ueber den Einfluss von optischem oder akustischem Reiz und grammatikalischer Form des Reizwortes auf den Assoziationsvorgang, *J. f. Psychol. u. Neurol.*, 23, 1918, 171-207.

for the uneducated. The number of educated (2 men and 1 woman) is too small for any conclusions to be drawn about them. The normal individuals of both sexes, however, find the visual stimulus harder to react to quickly, and the difference between their two averages is about 0.2 sec. The uneducated men have fewer complexes and fewer disturbances in reproduction; both sexes show more complexes and more disturbances with the auditory stimulus.

We may conclude by way of summary that the visual stimulus presents a more difficult problem for reaction than the auditory. The difficulty is perhaps barely greater for the women than for the men. The educated *Ss* show a reversed condition which, if it may be taken into account at all where only 3 *Ss*' performance is concerned, suggests that reading practice accounts for the reversal and might therefore explain the sex difference. The visual stimulus reaction type is (except for the educated members of the group) consistent in its effect of making the reactions more superficial because it acts as a distraction factor. Huber's research thus adds one new feature to the interpretation of sex differences in word association. It is the suggestion that the visual stimulus type offers a slightly more distracting element to the women than to the men. If we may accept this finding as valid we are, as before, still wanting any evidence as to the origin of the difference.

(*B*) *Verbal forms of presentation.* A detailed analysis of the influence of the grammatical form of the stimulus word upon the reaction is presented by Jung. Differences in terms of sex plus culture are rather large, but as far as they can be interpreted they appear attributable to response and personality type, which is mainly a function of habit and experience. If the men are compared with the women without respect to degree of education, the sex differences are negligible.

(*C*) *Free and controlled association.* A fairly consistent sex difference in response indicated that men are faster and show less variety and more community of interest in continuous association, while the contrary was found true in discrete association. With greater freedom of conditions the men's responses were more original and adequate; the women reacted more satisfactorily under more prescribed circumstances. This seems to accord with the expectation where two groups are being compared whose entire experience has discriminated between them in reference to the limits of freedom in activity.

The extreme case of prescribed conditions is seen in controlled association experiments, or tests, and here the comparison still holds good: the women tend to equal or exceed the men in successful performance. Haggerty and Kempf sufficiently illustrate this point for our survey since we are concerned here with free association only.

(*D*) *Influence of recording methods.* Two methods of response are usual: (a) oral, with or without some complicated timing apparatus, the response being then written down by the experimenter; and (b) written. The studies of the Jung group, including all those whose main purpose was diagnostic, were carried out by the first method. The Jastrow and Calkins studies and some other university and academic studies (*e.g.* Wissler, Thompson, and Pyle[67]) were carried out by the second method. No comparison of the two

[67] A. H. Pyle, *The Examination of School Children*, 1913, 24-27.

methods with reference to sex differences is known to us. A single point of interest stands out: whereas men's reaction-time was usually faster by the first method, women and girls wrote more words than corresponding groups of men and boys when the second method was used. This was usually the case whether the stimulus was oral or visual. Exceptions were found, as has been noted elsewhere by women *E*s, in the case of the oral responses and for certain groups in the written responses: by Pyle for older boys (17 and 18 year) and men, and by Wissler for university men. No definite sex difference is therefore regularly registered, although it would appear that men are usually somewhat quicker in oral response in a discrete series, while women are quicker in writing in a continuous series. We have discussed the question elsewhere in more detail.[68] We might add that the question, in the case of the written responses, may be one of motor facility rather than speed in mental process.

TABLE XIV

SEX DIFFERENCES IN RESPONSE TO COMPLEX INDICATORS (JUNG SERIES) VERSUS COMMON NOUNS (A & M SERIES)

(Computed from H. Huber's data for normal *S*s. Results expressed in percentages)

Response group	Men		Women	
	Jung	A & M	Jung	A & M
Inner	71.5	77.0	72.0	73.0
Outer	20.5	18.0	23.0	23.0
Clang	2.5	1.0	1.2	1.0
Residual	4.6	2.8	3.4	2.5
Egocentric	1.9	1.0	1.6	0.3
Time in secs.	2.3	2.4	2.8	2.7
Complexes	7.6	5.3	11.2	6.8
Disturbances in reproduction	25.1	18.3	23.9	14.7

(*E*) *Influence of high tension words.* Hans Huber's[69] comparison of (a) responses to the Jung series with (b) responses to the Aschaffenburg and Maier series throws into relief differences between associations to specific complex finders and those to common words of no specific emotional value (see Table XIV). In the Huber study the A & M words gave a more meaningful reaction type especially for the men: more inner responses, fewer outer, clang, residual and egocentric. The reaction-time for the Jung words was shorter for the men, longer for the women; the Jung words elicited more complexes for both and indicated more disturbances in reproduction. The response type differences for the sexes are all rather small; also the reaction-time differences. With respect to complexes and reproduction of earlier responses the differences are large, and indicate that the Jung series has a somewhat different effect from the A & M; it reveals lower strata of response in both sexes but conspicuously more for the women (60% increase, opposed to a 40% increase for the men). Perhaps this is to be explained as Jung explains the complex indicator, long reaction-time, in terms of the sex of the *E*.

[68] Miles and Terman, *op. cit.*, 1-31.
[69] H. Huber, *op. cit.*, 171-207.

Herrington and Willits found that women showed longer reaction-times to more words than did men. For both sexes the words responded to as complex indicators fell into definite fields of interest, and these differed for the two groups. The list was in fact, although without the investigators' intention, probably weighted to indicate more complexes for one sex than the other. The results of Hull and Lugoff, using a much longer list, showed an equal number of words that drew responses of high tension for the sexes, but again with typical content differences.

There seems to be here a question of (a) the choice of words that will elicit complexes within the masculine or feminine field of interest or mental occupation, and (b) the possible influence of the sex of the *E* which may heighten the tension of a whole series for the opposite sex. The Casons' results emphasize the latter point.

To available lists and under existing conditions, women, in the groups observed, appear to be relatively more sensitive than men; this effect is more noticeable the more diagnostic the words are of complexes. This seems to be due (a) to women's less objective response type, which is again perhaps connected with her more personalized experience, and (b) to the conditions of the experiment, where in general the *E* is a man. This related factor is further discussed in the next paragraph.

(*F*) *Influence of the E.* Sex differences due to the influence of the *E* that have been investigated and discussed pertain to reaction-time and to complexes. Jung found that women react more slowly than men, the uneducated than the educated, and although he made no control experiment with a woman investigator he traced the tendency for slower reactions primarily to emotional inhibitions and only secondarily to differences in culture: the *E* he said was in each case an educated man; for the educated men he was an individual of their own level and sex. For the uneducated men and for both groups of women he was either a person in authority placed over them, or else a person of the opposite sex, or both. This is a "sufficient reason for the universally stronger occurrence of emotional inhibitions in the remaining groups." Féré, H. Huber, Wreschner, H. Cason, Kramer and Stern[70] and other men investigators have, like Jung, found men's reaction-time in word associations shorter than women's.

Wells[71] concluded from his data that women's reaction-times grouped them into two classes, those whose responses were as short as or shorter than the men's, and those whose responses were subject to much greater deviation and averaged a longer time. This is probably accounted for by inadequate sampling. Hirszowicz[72] and W. Cason, women *E*s, found men's reaction-times longer than women's.

Herrington and Willitts found that men reacted more slowly with men *E*s than women with women *E*s in the same set-up. The Casons' data give the

---

[70]F. Kramer and W. Stern, Selbstverrāt durch Assoziation, *Beiträge z. Psychol. d. Aussage*, 4, 1906, 1-32.

[71]F. L. Wells, Some properties of the free association time, *Psychol. Rev.*, 18, 1911, 1-23.

[72]S. Hirszowicz, *Experimentelle Beiträge zur Analyse des Reproduktionsvorganges*, 1909, 13-24.

opposite result: both men and women responded more quickly (the men quickest of all) with a man *E*. The indications are therefore not quite clear in this matter, but tend in general to substantiate Jung's view that in word association responses men will usually react more quickly to men and women to women.

With respect to complexes conditions are similar, as in fact we should conclude, since response time is itself a complex-indicator. H. Huber, Burt and Moore, Rittershaus,[73] and Hull and Lugoff, using men *E*s, find more complexes indicated for the women than the men. Haggerty and Kempf, using *E*s of both sexes, and Herrington and Willitts, using men for the men and women for the women, still find more complexes in general among the women. We are inclined to believe that there is a slight demonstrable difference between the responses of groups from the two sexes, the more so because this would seem to accord with the trends which the mental habits of the sexes at present suggest.

In this connection the work of Kramer and Stern is of interest. These investigators of word association in 'crime detection' compared the responses of 4 men (3 university students and 1 teacher) with 3 women (teachers) and found a "noteworthy" difference between the sex groups. All 4 men gave themselves away by more or less suspicious responses, "of which frequently a single one was sufficient to indicate the presence of a complex." The women gave so few 'suspicious' responses that these played only an insignificant part in the diagnosis. It should be said that the responses of one of the women were in general so much out of line with the others that the comparison is perhaps of 4 men with 2 women, and upon such small numbers conclusions as to sex differences can hardly be based. However, while drawing attention to the doubtfulness of deduction in this case, the authors quote Hans Gross as follows: "I believe you are perfectly right in thinking that women dissimulate better. A woman has altogether more to conceal than a man: menstruation, pregnancy; her interest in men she has to hide more carefully than he does his in her; in conversation she has to be more careful (modest) etc.—in short she is more accustomed to it."

Aptekmann,[74] with the collaboration of Jung, compared the responses of 6 educated men and 6 educated women with a man *E* (Jung) and a woman *E* (Aptekmann). She found (a) that the male *E* produced greater affective changes in the *S*s, (b) that where the *E* and the *S* were of the same sex more social complexes appeared; where they were of the opposite sex more erotic complexes were indicated.

Sutherland[75] sums up the matter of the *E*'s influence critically as follows: "The situation is increasingly complex as the social and administrative relations between experimenter and test person widen. And since language is essentially a social function, the conditions of the reaction are by no means confined to those usually described in reaction experiments. . . . The situation presented to a young person serving as subject for the first time for his teacher or employer contains possibilities of variation which are quite lacking to the

[73]E. Rittershaus, Die Komplexforschung, *J. f. Psychol. u. Neurol.*, 15, 1909, 61-83, 184-220; 16, 1910, 1-43.

[74]E. Aptekmann, Experimentelle Beiträge zur Psychologie des psychogalvanischen Phänomens, *Jahrb. f. psychoanal. u. psychopathol. Forsch.*, 3, 1912, 591-620.

[75]A. H. Sutherland, *Critique of Word Association Reactions*, 1913, 1-46.

subject of experience, or one of equal social or administrative standing. The situation presented to the female subject when the experimenter is a male, and *vice versa*, can hardly be considered constant as compared with the situation presented to a young man by an intimate associate, and the constrained and embarrassed reactions of the younger subjects, as well as those of abnormal subjects, can hardly be interpreted with accuracy from a cold logical standpoint which disregards the experimental conditions."

In conclusion, it appears that complexes are more apt to be demonstrated in women than in men where men *E*s are in charge. With women *E*s the situation tends to be reversed, and furthermore the complexes tend to be somewhat different in character. The reversal is, however, not quite clear-cut, and it would appear that some other factor may be involved, related perhaps to the response or personality types discussed above, *i.e.* the result of some such more or less habitual attitudes as Gross suggested.

Conclusion

We have surveyed the reported differences between the sexes in the association of ideas as demonstrated by traditional types of the word association technic. From this extensive comparison two well-established facts emerge: (1) significant sex differences in the quality of word associations as indicated (a) by their content (type, extent, value, normality) and (b) by their form, are found in a number of cases where the groups compared are unequal in experience; they are not found in the few cases where the training and experience of the groups was approximately similar; and (2) considerable differences are found to exist between the respective fields of interest of men and women, and of boys and girls, who are representative of the general population.

Sifting the data systematically has brought to light the following unlikenesses between the sexes in terms of the responding *S*s. (A) There is a slight tendency to difference in response type expressed in terms of more superficial and less meaningful (male) or less indifferent and more coöperative (female) responses. (B) A slight tendency to difference is found in that personality type which appears to be rather more a function of the individual and less of the performance. The male characteristics are objectivity, logic, cool judgment; the female characteristics are subjectivity, personal evaluation, warm appraisal. But when the matter is minutely studied it appears that the personality type is (a) partly a deeper aspect of the response type, and (b) an individual character which is present oftener within one sex than the other in the groups studied, but is not universal in either case and hence not

demonstrably an innate sex trait. (C) Differences slight and great exist in interest type as viewed from two aspects: first, extent of interest, value of interest, normality of interest, and second, character of interest. With reference to the first it appears that sex differences are quite negligible when groups of adequate size and equality of status are compared. With reference to the second it is equally evident that wide differences prevail similar to those found when occupationally variant groups within a single sex are compared. (D) Insignificant differences only are found in the form type, whether verbal merely or logical in character. These appear to fall under the influence of the response type tendency, that is, to be largely a matter of experience and habit in verbal or logical expression.

Differences in terms of the stimulus are also present in the groups compared and these again show two basic aspects. (A) A slight unlikeness is found in response to two modes of presentation. (B) Negligible differences are present where the verbal form of presentation is concerned. (C) A difference appears in speed of response, dependent on the degree of definite specification as to procedure. (D) A difference in speed of response is present depending on the vehicle of response. (E) A difference is demonstrated in the degree of affect produced by more or less emotionally tinged words. (F) A difference appears in speed and character of response dependent on the sex of the *E*. Each sex appears to have an effect on both its own and the other sex, these effects differing each from each according to customary and readily distinguishable social tendencies.

The cumulative evidence indicates that the groups of women who have been compared with the groups of men show a slightly more introverted, subjective, evaluating type of response. This would be expected from any group, without regard to sex, that had been subject to the somewhat more personalized and limited life experience of these women as compared with the men. The extent, value, normality of interest, and the form in which it is expressed do not vary for the sexes appreciably except as they are conditioned by the character of the interest expressed.

Are there sex differences in the association of ideas as revealed by the usual word association technic? Although this question must be answered in the affirmative, it has not been proved that these are traceable to primary innate differences.

It is possible that they merely reflect present social custom and practice. At any rate, it is certain that statements with reference to sex differences observed in casually selected groups are likely to be quite misleading, and that results and conclusions are important only in terms of the conditions. Whether such sex differences as are found to be demonstrable are chiefly native or chiefly acquired is a question which can only be answered in terms of investigational data now almost entirely lacking. When the actual differences have been more clearly determined by improved methods, it will be possible, where the differences are of any considerable magnitude, to secure a quantitative measure of the influence of various environmental factors upon them.

In a later contribution the writers will show that it is possible to construct a word association test that brings out measurable sex differences whatever unselected groups are tested. The stimulus words used have been chosen by actual trial as those that showed clear tendencies in the direction of sex differences in response. A test composed of 70 stimulus words so selected shows in representative groups under existing social conditions but a moderate amount of overlapping between the sexes. That the demonstrated differences are indicative of divergent factors in the interests of sex groups is probable; it is even possible that individual personality differences may be indicated, but to what extent these are due to nature and to what extent to nurture is still a matter of conjecture. The final answer will have weight only as it is the result of adequate investigation.

THE RANGE OF ATTENTION

By A. Douglas Glanville and Karl M. Dallenbach, Cornell University

Though the problem of the 'range of attention' has a history that extends back to Aristotle, the question whether it is a proper problem to set to attention has only recently been raised. Dallenbach in 1920[1] and Fernberger in 1921[2] came to the conclusion that it is not. Both expressed the opinion, based upon experimental evidence, that the traditional problem of 'range' is one that is concerned with apprehension, not with attention.

From Aristotle to Hamilton the question of 'range of attention' has been discussed and decided upon logical and empirical grounds; and the answers returned have varied with their day and generation. Aristotle asserted that the mind is limited to the consideration of a single object,[3] *i.e.* that attention is limited to one. This dogma held for nearly 400 years, until it was rejected by Quintilian[4] who argued, from such examples as the harpist who sings as he plays his instrument, that it is possible to attend at the same instance to several sensations and objects. The schism thus made has held down to the present century.[5] The Scholastics were divided upon the question, as were also the philosophers following them. Hobbes, Locke, Wolff, Stewart, and Brown, for example, held to the Aristotelian doctrine, whereas Leibnitz, Tucker, Bonnet, and Destutt de Tracey opposed it.

Hamilton made an attempt to solve the problem by experiment. He threw "a handful of marbles on the floor" and because he found "it difficult to view at once more than six,"[6] he came to the conclusion that the constant of attention was six. Since this experiment of nearly 100 years ago, the technique has been improved, tachistoscopes have been employed to control the time of exposure, many different kinds of stimulus-objects have been used, and the experiments have been extended into the fields of audition and of touch.[7]

---

*Accepted for publication June 15, 1928.

[1] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 229.

[2] S. W. Fernberger, A preliminary study of the range of visual apprehension, this Journal, 32, 1921, 133.

[3] Aristotle, *Parva Naturalia, De Sensu*, chap. vii, Eng. trans. W. A. Hammond, 1902, 186 ff. Cf. also L. L. Uhl, *Attention*, 1890, 5.

[4] Quintilian, *Institutes of the Orator*, Book I, chap. xi, Eng. trans. J. Patsall, 1774, Vol. I, 66. Cf. L. L. Uhl, *op. cit.*, 7.

[5] H. Ebbinghaus, *Grundzüge der Psychologie*, 1905, 618 ff.

[6] Wm. Hamilton, *Lectures on Metaphysics and Logic*, Lect. xvi, 5th ed., I, 1870, 254.

[7] For a bibliography of the more important studies see N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this Journal, 37, 1926, 247 f.

The different investigators, as Gill and Dallenbach pointed out,[8] have required reports from their *Os* of very different levels of cognition. Some of the investigators have asked their *Os* to report merely the number of stimulus-objects exposed, others have instructed their *Os* to name the stimulus-objects, while still others have required their *Os* to describe the stimulus-objects. In every case the 'range' has been determined by the number of stimulus-objects correctly apprehended under the conditions of the particular experiment.

Report, even in those cases in which the *Os* were asked to report merely the number of stimulus-objects exposed, implies cognition. Unless, therefore, it can be definitely established that cognition and attention are co-variables, the claim that these experiments give the 'range of attention' is not warranted. The justification for the claim rests, as Dallenbach pointed out,[9] upon two assumptions, "(1) that all processes attended to are reported; and (2) that all processes reported have been attended to."[10] Dallenbach showed that neither of these assumptions was correct: processes attended to were not always reported, nor had the processes reported always been attended to. There were, he found, many other factors besides attention that conditioned report. He was consequently led to the conclusion that the "experiments upon the range of attention therefore give us merely a range of cognition, and the statement in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention."[11] From different premises—his *Os*' introspections—Fernberger arrived at the same conclusion. He says, at the end of his study, "from a consideration of the introspections we are convinced that the range of attention is an erroneous title for this sort of experiment."[12]

These attempts to set the problem of 'range' outside the field of attention gave rise to serious efforts to save the traditional experiment.

Oberly sought, by adding introspections to his *Os*' tasks, "to differentiate the thresholds of attention, cognition, and apprehension."[13] He found that his *Os* distinguished three conscious patterns in the tachistoscopic work: the "immediate," in which all the stimulus-objects (dots) were perceived in a single flash of consciousness with an equal and high degree of clearness; the "grouping," in which all the dots were seen in a single flash of consciousness, but in which there was, because of different degrees of clearness, an attentional grouping of the stimulus dots; and the "counting," in which the report was always made on the basis of some subsequent re-imaging of the stimulus material. Constants were computed by the method of constant stimuli for the range of

---

[8]N. F. Gill and K. M. Dallenbach, *op. cit.*, 248.

[9]K. M. Dallenbach, *J. Exper. Psychol.*, 3, 1920, 228 f.

[10]K. M. Dallenbach, Dr. Fernberger on the 'range of attention' experiment, this JOURNAL, 38, 1927, 479.

[11]K. M. Dallenbach, *J. Exper. Psychol.*, 3, 1920, 229.

[12]S. W. Fernberger, this JOURNAL, 32, 1921, 133. Bentley, for systematic reasons, arrives practically at the same point. He says, "the 'range' of attention [is] a topic which properly belongs in a discussion of the limits of psychosomatic function," and he treats of it under that heading (M. Bentley, *The Field of Psychology*, 1924, 177, footnote 3).

[13]H. S. Oberly, The range for visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 333.

every one of these patterns. The range was smallest in magnitude for the 'immediate' pattern, intermediate for the 'grouping' pattern, and greatest for the 'counting' pattern. Because these patterns did not correspond to any of the systematic categories of psychology, Oberly transformed them into 'attention,' 'cognition,' and 'apprehension' respectively; and he carried his results and conclusions from these introspective patterns directly over to these systematic categories. He thought by the alchemy of his transformation to save the problem of range for attention. His work was followed by several other studies in which his premises and 'transformations' were accepted without question.[14]

The results of Oberly's experiment and of those that followed from it do not, however, save the problem for attention. The results of this series of studies corroborate, on the whole, Dallenbach's and Fernberger's contentions, for the 'transformation' of the 'immediate' pattern into 'attention' involves at the same time a transformation of the term attention. In Oberly's hands attention becomes a cognitive term—the highest degree of cognition with which he was concerned. This identification, as Dallenbach pointed out in his critical note regarding Oberly's experiment,[15] is both incorrect and misleading. It is incorrect because attention is not a cognitive term—even the functional systems of psychology do not regard attention as a level or degree of cognition; and it is misleading because Oberly's study does not concern attention—attention transcends the cognitive field in which his study was made. Oberly's attempt to differentiate the threshold of attention does not, therefore, save the traditional experiment; his study does not bear upon the problem.

A second effort to save the traditional experiment was made by Gill and Dallenbach,[16] who attempted to set the problem so as to escape the cognitive complications of the earlier studies. They thought to accomplish this by asking their *O*s to report not upon the stimulus-objects but upon the distribution of the attensity (clearness, vividness) of the impressions during the moment of the exposure. Cards with from 3–15 stimulus-objects were exposed tachistoscopically for 60 sigma. Every stimulus-number was exposed 100 times. Limens of the one-level clearness reports, computed from these data by the method of constant stimuli, extended for the different *O*s from 17–42 stimulus-objects. Though these constants were contaminated to the least possible degree by cognition, Gill and Dallenbach were tentative in their conclusions. The stimulus-values they used were unfortunately selected:[17] the distribution

[14]H. S. Oberly, Further results in the 'range for visual attention, cognition, and apprehension' experiment, *ibid.*, 37, 1926, 132-137; A comparison of the spans of 'attention' and memory, *ibid.*, 40, 1928, 295-302; S. F. Cooper, The effect of brightness in the range of attention experiment, *ibid.*, 40, 1928, 254-274; J. R. Hart, Jr., The range of visual attention, cognition, and apprehension for colored stimuli, *ibid.*, 40, 1928, 275-283.

[15]K. M. Dallenbach, Dr. Oberly on 'the range for visual attention, cognition and apprehension,' *ibid.*, 36, 1925, 154 ff. Cf. also, Dr. Fernberger on the 'range of attention' experiment, *ibid.*, 38, 1927, 479 ff.

[16]N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, *ibid.*, 37, 1926, 247-256.

[17]They thought, in view of the fact that the traditional constant fell at 4–6, that the critical range in their work would fall somewhere between their extremes. As it turned out they simply 'guessed' wrong.

of the *Os*' reports of one-level attensity were badly skewed with reference to them—limens in every case exceeded the largest stimulus-value used. The variation in the size of the limens among the different *Os* was, moreover, unusually large. It was perfectly obvious, therefore, that Gill and Dallenbach's work must be repeated with introspections and with an extended range of stimuli before any but provisional conclusions could be drawn.

Object

The object of the present experiment is twofold. We wished in the first place to repeat Gill and Dallenbach's work, adding introspections and extending the range of the stimulus-values; and in the second place to demonstrate that the constant of the traditional 'range of attention' experiment is dependent upon the degree of cognition involved in the report, and that the range of *attention*—if it be possible to formulate such a problem—can be properly investigated only when the cognitive factor is eliminated.

Procedure

Five series of tachistoscopic experiments were performed; the *Os* being differently instructed in every series. In the first series, Experiment I, in which we sought to escape the cognitive complications of the traditional experiments, we instructed our *Os* to report merely upon the distribution of the attensity of their impressions during the moment of exposure. In the remaining series, Experiments II–V, the cognitive complications involved in report were taken into account. We induced different levels or degrees of cognition by instructions and observed how the 'range' of the reports was affected. We instructed the *Os* in one series to give the number of stimulus-objects exposed, in the next series to name the letters exposed, in the next series to name the forms exposed, and in the last series to describe the forms exposed. We began our study with a series of experiments that did not involve cognition (at least not the cognition of the stimulus-objects), and progressed to series that demanded higher and higher levels of cognition for the reports, because we thought that the 'habits' of report would be weaker and less inhibitory if we passed from lower to higher degrees of cognition than if the progression were reversed.

Observers. The *Os* were Miss Alexandra Hobart (*H*), a senior majoring in psychology; Mr. Tallmadge Griffin (*G*) a graduate student in psychology, and the senior author (*D*). They served three times a week and at the same hour on the different days. *D* was an *O* in Gill and Dallenbach's study and he had

also served in a number of other studies on attention. He was the only one of our *Os* who came to the study introspectively familiar with the dimension of attensity.

Preliminary experiments. Because *G* and *H* were introspectively unfamiliar with attensity (attributive clearness, vividness) we gave them a series of preliminary experiments which had been devised in the Cornell laboratory for training in the introspection of it.[18] The experiments were continued until the *Os* were able to note the grosser variations of attensity; until they were able, for example, to characterize certain impressions as more insistent, protruding, clearer, or more vivid than others. When this point had been reached we turned to the main experiments. *H* quickly reached this degree of proficiency, but *G* was more perverse.[19] It was only after long practice that he gave any indication that he appreciated the attensive differences in experience.

While *G* and *H* were being given the preliminary training, *D* was observing in the main experiments. The first series of the main experiments (Experiment I)—those involving the report of attensity—were given to *D* twice over before *G* and *H* were ready for them. The first time they were given with a dark post-exposure field; the second time with a light post-exposure field identical with the pre-exposure field.

These experiments were conducted in order to discover whether the quality of the post-exposure field would affect the results. We wished to know, so as to be guided in conducting the experiments with the other *Os*, whether there were fewer or more positive and memory after-images with a light than with a dark post-exposure field, and also whether the presence of these after-experiences would render reports of attensity more or less difficult.

The results of these two series of experiments with *D* are given in Tables IA and IB below. There is very little difference between the results of the two series; but what difference does exist tells in favor of the light post-exposure field. The objective results show that the limen is slightly larger and the precision slightly greater when the light field was used. *D*'s introspections also show that the light field was probably the better. He reported that positive and memory after-images were more intense and of more frequent occurrence when the dark post-exposure field was used than when the light field was used. Though he was of the opinion that these after-images affected neither his experience nor his report, their increase in intensity and frequency was sufficient

---

[18] Cf. K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 467.

[19] *H* came to the preliminary experiments merely naïve in the introspection of attensity, but *G* came to them with a certain amount of sophistication and a prejudice against the concept of attensity as an attribute or dimension of experience. It may have been unwise to have included among our *Os* one of such strong persuasions who was thus negatively conditioned, but we gladly accepted him as a challenge. The selection of *Os* is doubtlessly justified, but it is our opinion that it often leads to the stereotyping of results, and to the indefinite postponement of important discoveries. Anomalous reports of 'nonconforming' *Os* have frequently in the history of psychology been of more consequence than the routine reports of the 'regular' *Os*.

to prejudice us against the dark field. We therefore used the light post-exposure field for *G* and *H* in Experiment I, and for all the *O*s in the remaining experiments (Experiments II–V).

Apparatus. For the exposure apparatus we used throughout all five series of experiments the Dodge tachistoscope,[20] together with a modification of Crosland's timing device.[21] The time of exposure was approximately 78 sigma.[22] As described by Crosland his apparatus gives a dark post-exposure field. As this was found to be undesirable (cf. *supra*) we modified the apparatus to give a light post-exposure field identical with the pre-exposure field.

Materials. The exposure-cards were 11.5 x 5.5 cm.—the size adapted to the Dodge instrument. They were white, with a soft, dull surface. The stimulus-objects were black (with the exception of those used in Experiment 5, which were colored) and (with the exception of those used in Experiment 3, which were letters) they consisted of small geometrical forms which were cut out of the cards by punches. When the cards were placed in the holder, which was painted a dead black, the stimulus-objects were uniform in appearance and sharp and definite in outline.

Care was taken in making the cards to eliminate as far as possible the effect of position and of pattern. The stimulus-objects were distributed evenly over the exposure-field, and the cards which showed an objective grouping were rejected.

Experiment I

We hoped in Experiment I to escape the cognitive complications of the traditional experiments by instructing our *O*s to report, not upon the number or kind of stimulus-objects observed, but upon the distribution of the attensity of their experience, *i.e.* whether the impressions were at one, two, or more levels of attributive clearness. What we sought in this experiment was the answer to the question how many impressions may, in a single consciousness, be experienced at the upper level of attributive clearness; or to put the question more concretely: what is the limen of the upper level, how many stimulus-objects may be placed upon an exposure-card before they are experienced 50% of the time at two or more levels of clearness.

Exposure Cards. Ten series of exposure-cards were prepared in the manner described above. Every series was composed of six cards which had respectively 3, 8, 13, 18, 23, and 28 stimulus-objects. In the first nine series (Series A–I) a single form was used in a given series; in the tenth series (Series J) the forms

[20] R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.
[21] H. R. Crosland, A device for controlling the time of exposure in the Dodge tachistoscope, *J. Exper. Psychol.*, 9, 1926, 162-168.
[22] The average of 10 trials was $78.29 \pm 2.27\sigma$.

were all different in the cards with 3, 8, and 13 stimulus-objects, and no form was used more than twice in the cards with 18, 23, and 28 stimulus-objects. The forms used in Series A–I were the same as those used by Gill and Dallenbach[23]—they are listed in Table IA below. In Series J all the forms of the first nine series and five new ones were used.[24]

There were thus 10 cards for every stimulus-value or 60 exposure-cards in all. The cards were shuffled and presented in haphazard order, and the procedure was repeated 10 times, making a total of 100 observations for every stimulus-value, or 600 observations for very *O*.

Instructions. The following instructions, identical with those used by Gill and Dallenbach, were read to the *O*s at the beginning of every experimental hour.

"At the signal 'Ready,' fixate the black dot on the pre-exposure field. At 'Now' you will be shown a white field upon which there is a number of black figures. After the experiment report upon the attensity, that are the clearness or vividness of the impressions, during the exposure period. You may use the following terms: 'one' or 'equal,' meaning that all the impressions were at one level of attensity; 'two' or 'different,' meaning that two levels were observed; and 'three' or 'multiple,' meaning that three or more levels were observed."

Doubtful judgments were permitted, but the experiments in which such reports were given were later, without *O*'s knowledge, repeated. The experiments were interrupted at irregular intervals and the *O*s were asked to give an introspective report upon their experience during the exposure-period. They were also instructed to interrupt the course of the experiments any time they had anything important to report.

Results. The *O*s' reports were either 'one' or 'two.' Not once in the entire experiment was the report of 'three' returned, though it was, as in the earlier study by Gill and Dallenbach, tacitly assumed and provided for in the instructions. The *O*s' introspections show that their experiences, during the moment of exposure, were divided into two levels of attensity: a clear focus and an obscure background.[25] In the experiments in which the report of 'one' was given, the impressions on the exposure-card were all experienced at the upper level. At the lower level there were certain obscure kinaesthetic and organic sensations and images. When 'two' was reported, the impressions from the exposure-cards were themselves divided into two levels of attensity: some were

[23] *Op. cit.*, 250.

[24] Greek cross, heart, rectangle, angle, and an irregular form that the *O*s called a 'crown.'

[25] Cf. E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 220-242; K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 506 f; Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 225-227; N. F. Gill and K. M. Dallenbach, *op. cit.*, 252.

TABLE IA

SHOWING THE NUMBER OF TIMES THAT THE IMPRESSIONS FROM THE VARIOUS STIMULUS-SERIES WERE REPORTED AT ONE LEVEL OF ATTENSITY, THE TOTAL NUMBER OF THESE REPORTS FOR EVERY FORM-SERIES, THE AVERAGE, AND THE DEVIATION FROM THE AVERAGE

O	Post Exposure field	Form Series	Forms	Number of Stimulus-objects 3	8	13	18	23	28	Total Form-Series	Dev. from Av.
		A	Small disks	10	10	5	3	2	3	33	− 1.6
		B	Medium disks	10	10	8	2	1	1	32	− 2.6
		C	Large disks	10	8	7	1	3	3	32	− 2.6
		D	Semi-disks	10	10	10	9	2	0	41	+ 6.4
		E	Triangles	5	10	8	2	5	0	30	− 4.6
D	Dark	F	Clubs	10	6	6	2	1	2	27	− 7.6
		G	Diamonds	10	7	7	7	2	3	36	+ 1.4
		H	Squares	10	6	7	3	3	2	31	− 3.6
		I	Spades	10	9	9	7	1	3	39	+ 4.4
		J	Combinations	7	10	9	9	6	4	45	+10.4
			Total	92	86	76	45	26	21	Av. 34.6	± 4.5
		A	Small disks	10	9	7	7	0	1	34	− 3.1
		B	Medium disks	10	10	8	7	1	2	38	+ 0.9
		C	Large disks	10	8	8	4	1	1	32	− 5.1
		D	Semi-disks	9	10	10	6	7	3	45	+ 7.9
D	Light	E	Triangles	9	10	5	3	3	2	32	− 5.1
		F	Clubs	10	10	8	0	2	0	30	− 7.1
		G	Diamonds	10	9	9	3	1	2	34	− 3.1
		H	Squares	10	8	9	5	3	1	36	− 1.1
		I	Spades	10	10	8	6	1	2	37	− 0.1
		J	Combinations	10	10	10	7	8	8	53	+15.9
			Total	98	94	82	48	27	22	Av. 37.1	± 4.9
		A	Small disks	10	10	9	10	10	10	59	+ 0.5
		B	Medium disks	10	9	9	10	10	10	58	− 0.5
		C	Large disks	10	10	10	9	10	10	59	+ 0.5
		D	Semi-disks	10	10	10	9	10	10	59	+ 0.5
		E	Triangles	8	10	10	10	10	10	58	− 0.5
G	Light	F	Clubs	9	9	9	10	10	9	56	− 2.5
		G	Diamond	10	10	10	10	10	10	60	+ 1.5
		H	Squares	10	10	10	10	10	9	59	+ 0.5
		I	Spades	10	9	9	10	10	10	58	− 0.5
		J	Combinations	10	10	10	10	9	10	59	+ 0.5
			Total	97	97	96	99	98	98	Av. 58.5	± 0.8
		A	Small disks	8	5	6	9	5	5	38	+ 7.1
		B	Medium disks	8	5	5	3	2	4	27	− 3.9
		C	Large disks	8	7	8	9	9	8	49	+18.1
		D	Semi-disks	5	2	2	6	6	3	24	− 6.9
		E	Triangles	6	0	9	4	10	6	35	+ 4.1
H	Light	F	Clubs	0	1	3	6	6	6	22	− 8.9
		G	Diamonds	6	9	7	5	7	5	39	+ 8.1
		H	Squares	1	3	5	6	1	2	18	−12.9
		I	Spades	2	6	2	3	6	6	25	− 5.9
		J	Combinations	6	0	4	7	8	7	32	+ 1.1
			Total	50	38	51	58	60	52	Av. 30.9	± 7.7

clear, others were obscure. Since these two kinds of reports are reciprocal, we give in Tables IA and IB the number and distribution of the reports of 'one' only.

Table IA shows the number of times that the impressions from the various stimulus-series were reported at one level of attensity; Table IB gives the percentages of the one-level reports for every stimulus-value, and the limens and measures of precision for the two series of experiments with *D*—the only one of our *O*s for whom a limen could be calculated. The results are very different for the three *O*s; we must, therefore consider, them separately.

TABLE IB

SHOWING FOR EVERY *O* THE PERCENTAGE OF ONE-LEVEL REPORTS FOR EVERY VALUE OF STIMULUS, AND THE LIMEN (L), AND THE MEASURE OF PRECISION (h) FOR THE TWO SERIES OF EXPERIMENTS WITH *D*

	Number of Stimulus-objects							
O	3	8	13	18	23	28	L	h
*D**	92	86	76	45	26	21	18.2	0.07
D†	98	94	82	48	27	22	19.4	0.08
G	97	97	96	99	98	98	——	——
H	50	38	51	58	60	52	——	——

*Dark post-exposure field †Light post-exposure field

The frequency of *D*'s one-level reports varies, in both series of experiments conducted with him, with the number of stimulus-objects exposed. The curves of his reports are smooth and regular, with no inversions of the first order.[26] The limens, calculated from these data by the method of constant stimuli, are 18.2 for the experiments with the dark post-exposure field, and 19.4 for those with the light post-exposure field. These values agree closely with the liminal value, 17.3, obtained for *D* in Gill and Dallenbach's earlier study.[27] Do these 'constants' represent *D*'s 'range of attention'? It is obvious that his reports are conditioned by the number of stimulus-objects on the exposure-cards, but we must, before we are justified in drawing this conclusion, be assured that 'number' was the only condition that affected his reports. The introspections do not help us to determine this point, for

[26]These results sustain the opinion expressed by Gill and Dallenbach (*op. cit.*, 252) that a smooth psychometric curve would be obtained in their experiments if the number of stimuli were greatly extended above 15, and if stimulus-values were selected which differed by steps of 5 instead of by 1.

[27]*Op. cit.*, 252. The agreement is surprisingly close when it is recalled that the value in the earlier study was calculated from an upper section only of the psychometric curve.

they were given over to the description of consciousness after it had been conditioned; they were not concerned with the conditions. The fractionated data, shown in Table IA, clearly indicate that something besides 'number' of stimulus-objects was affecting *D*'s reports. The exposure-cards of certain of the Form-Series are reported more frequently as of 'one-level' than the cards of others, and the differences are too large to be due to chance. For example, in the experiments with the dark post-exposure field, 'one' was reported 45 times with Series J and 27 times with Series F; in those with the light post-exposure field, 'one' was reported 53 times with Series J and 30 times with Series F. It is clear, therefore, from external evidence alone, that these data are heterogeneous, that in spite of all our precautions the reports in some of the Form-Series were differently conditioned from those in others. How they were conditioned, otherwise than by 'number,' we do not know; and it is a problem that need not now concern us, for we may without knowing the cause of the heterogenity of our data, obtain homogeneous data (on the principle that results heterogeneously determined will show large variations) by omitting from final consideration those Form-Series whose total number of one-level reports deviates from the average reported for all the Form-Series by an amount greater than the m.v. The totals of Series D, E, F, and J, in the experiments with the dark post-exposure field, and of Series C, D, E, F, and J, in the experiments with the light post-exposure field, deviate from the average by such amounts; consequently those series were omitted from final consideration. The limens computed from the selected data are 17.3 and 18.6 respectively for the two series of experiments. Now, do these corrected 'constants' represent *D*'s 'range of attention'? Which, if either, since they differ by 1.3, a significant amount, should we accept? Or must we conclude that there is a constant for a dark post-exposure field, and another constant for a light post-exposure field?[28] Before we come to conclusions, let us examine the results of the other *O*s.

The results of *G* and *H* are, as we have seen, very different from those of *D*. The frequency of their one-level reports does

[28]We know from the experiments in apparent movement that experience is not stable, that it is altered by what follows it, and so we need not be surprised to learn here that the experience during the moment of exposure is affected by the post-exposure period.

not vary with the number of stimulus-objects exposed. The curves are practically horizontal straight lines, so that it is impossible to calculate limens from them. In every other respect, however, the reports of *G* and *H* differ. *G* reported 'one' in 97.5% of the experiments; *H* in 51.5%. *G*'s reports are equally distributed, as Table IA shows, among the different Form-Series as well as among the different Number-Series. *H*'s reports vary greatly among the different Form-Series, ranging from 49 in Series C to 18 in Series H. *G*'s results indicate that all the experiments were similarly conditioned for him; *H*'s results indicate that some of the experiments—those with Series C, F, G, and H in which the total number of one-level reports deviated from the average for all the Series by amounts greater than the m.v.—were differently conditioned for her. The distribution of *H*'s one-level reports among the different Number-Series was not, however, affected by the results of the heterogeneously conditioned series, for the distribution, when those Series were omitted from consideration, was approximately the same as before—the frequency curve was still practically a horizontal straight line.

It is perfectly clear from our data that objective conditions were not affecting the reports of *G* and *H*. What subjective factors were affecting them, we are unable to say. The introspective reports of the *O*s do not, unfortunately, help us; they enabled us to keep a constant check upon the *O*s—to know what the *O*s meant by 'one' and 'two'—but they tell us nothing about the conditions affecting the reports.[29] Our knowledge of the subjective conditions is limited therefore to the objective results; and the objective results tell us little. The frequency of the one-level reports do indicate, however, the constancy of the subjective conditions. Since *G* reported one-level in 97.5% of the experiments, and *H* in 51.5% of them, it appears as though *G* were 'set' for the report of 'one,' and as though *H*'s reports were determined by chance.

To explain the results of one *O* by 'set' simply means that the results of the other *O*s may be similarly explained. They have a

[29]The *O*s were asked to give attensive descriptions because the attensive aspects seemed, in view of the results of Gill and Dallenbach's preliminary study, the most important to be reported for our purposes. We could not foresee the development of the experiments, and we did not discover the need for attitudinal instructions until after the completion of the experiment when the data were examined and studied for the first time.

different 'set.' Unless, therefore, 'set' be specifically defined in every case explanation is bankrupt. Our results tell us nothing about 'set' beyond the fact that it differed among our *O*s. We are, consequently, unable to explain our results. An explanation is not, however, necessary or even pertinent to the purpose of the present experiment. The significant thing for us is that the *O*s, reports of 'one' were differently conditioned. That fact—with its corollary, that 'number' of stimulus-objects exposed was not a compulsory condition, indeed, that for two of our *O*s it was not even a contributory condition—indicates that 'range' is not a question that can properly be asked of the attentive integration in these experiments. The *O*s' attitude or 'set,' not the 'number' of stimulus-objects exposed, determined the report returned.

Experiment II

We sought in the second series of experiments to determine the number of stimulus-objects that could be apprehended correctly. We thought that the report of mere 'number' would involve as low a degree of cognition as we should be able to attain in the tachistoscopic experiment.

Exposure-cards. Ten series of 13 exposure-cards, whose stimulus-values ranged from 3–15 stimulus-objects, were prepared in the manner described above. The stimulus-objects, with one exception, were the same as those used in Experiment I.[30] A different form was used in every one of the first nine series. All those forms and six additional ones were used in the tenth series.[31] Care was taken in making the cards of the tenth series to see that no form was used more than once on any given card, and that every form was used on the different cards approximately as often as every other form.

The cards were shuffled and presented to the *O*s in haphazard order, and the procedure was repeated 10 times. Thus 100 reports were obtained for every value of stimulus—a total of 1300 reports for every *O*.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour.

"Cards will be exposed which contain a varying number of black forms on a white background. Immediately after the exposure report the number of forms you have apprehended. Be sure of your judgment; do not guess. The report, 'I do not know,' is admissible."

At the end of every experimental hour the *O*s were asked to give an introspective account of their experience during the exposure and of the conditions

[30]A small disk with a projecting bar was substituted for the large disk. The *O*s designated this figure as 'Saturn.'

[31]The five additional forms used in Experiment I and a new form, a disk with a white band, which the *O*s called 'Globe.'

underlying their subsequent reports. They were also told to interrupt the course of the experiments—which they did—whenever they wished to give an introspection.

Results. We give the objective results in Table II, which shows, for every *O*, the frequency with which the number of stimulus-objects exposed was correctly apprehended, together with the limens and measures of precision as calculated by the method of constant stimuli.

TABLE II

SHOWING THE FREQUENCY WITH WHICH THE *Os* CORRECTLY REPORTED THE NUMBER OF STIMULUS-OBJECTS EXPOSED, THE LIMENS (L), AND MEASURES OF PRECISION (h)

O	Number of Stimulus-objects													L	h
	3	4	5	6	7	8	9	10	11	12	13	14	15		
D	100	100	100	100	97	84	83	74	68	58	5	8	3	11.3	0.30
G	100	98	78	49	38	3	2	4	0	0	0	0	0	6.2	0.44
H	100	100	100	98	98	63	55	16	3	2	0	0	0	9.2	0.53

Though the *Os*' limens differ greatly in size, being 6.2, 9.2, and 11.3 for *G*,[32] *H*, and *D* respectively, the frequency curves of their reports are similar except for the aspect of spread. The curves of all the *Os* show a slow and gradual decline as the number of stimulus-objects increases until a point is reached—at 13 for *D*, 8 for *G*, and 10 for *H*—at which there is a sharp break. Both the size of the limen and the sharp breaks in the frequency curves are due, we believe, to the methods that the *Os* used in arriving at a report.

When the stimulus-objects were few in number, the impressions were apprehended as separate units and the reports were immediately returned: that is, as the following introspections show, the reports were touched off by the 'given' without the mediation of other conscious proceeses.

D, [3 stimulus-objects exposed; 3 reported]: "Judgment immediate, based upon impression which was at upper level of clearness." [4 stimulus-objects exposed; 4 reported]: "Impressions equally clear, and immediately apprehended. Memory after-image did not appear until after the report."

H, [4 stimulus-objects exposed; 4 reported]: "Lower two forms clearer than upper. Did not have to count them, that is, answer immediately given by exposure."

When more stimulus-objects were exposed the impressions were apprehended as a group, and the reports were returned upon the basis of this group apprehension without the mediation of other experiences.

[32] For a further discussion of *G*'s limen in this experiment see p. 223 f.

D, [5 stimulus-objects exposed; 5 reported]: "Impressions equally clear during exposure. A group of 5, apprehended in the form of a dipper: 4 dots the bowl, and a fifth dot the handle." [6 stimulus-objects exposed; 6 reported]: "Equally clear; grouped in single figure which extended across exposure field."

G, [general statement]: "The figures fall at times into patterns which, since I know the number of figures required to make the pattern, give me the meaning of the number exposed."

H, [5 stimulus-objects exposed; 5 reported]: "Did not count them; came clearly as a familiar form."

When a large number of stimulus-objects were exposed, the reports were based upon the organization of the post-exposure experience—probably the memory after-image.[33] The impressions were divided into two or three groups and the number of separate impressions was counted or the total number was obtained (if the number in each group was given with the group) simply by the process of addition.

D, [9 stimulus-objects exposed; 10 reported]: "Impressions equally clear during exposure. They were not arranged in a pattern during the exposure and they stood unaccented upon the field. After exposure memory after-image appeared; it was grouped. Upon basis of this grouping gave judgment 10. There were two groups of 3, and one group of 4. Answer 10 given after the three groups were mentally summated." [10 stimulus-objects exposed; 10 reported]: "Impressions equally clear. Report made upon basis of the organization of the memory after-image. There was a group of 2 dots at extreme left, of 3 in the middle, and of 5 at right. Judgment 10 given after adding the number at right to the number at left and adding to this total the number at the middle, that is 2–7–10." [14 stimulus-objects exposed; 14 reported]: "Impressions during exposure were all at the upper level of clearness. Memory after-image appeared; it was clear and organized into three groups. A group of 4 at left, of 4 in middle, and of 6 at right. Answer 14 a mere matter of summation."

G, [general statement]: "Do not think that the judgment is often made during the exposure except when figures are few in number, or patterns are familiar. Otherwise I have meaning of pattern left from exposure, and from that I figure out the number of forms on the card. Quite often the figures are organized into groups, and judgment is made by adding together the number of figures in each group. There seem to be two kinds of organization: simple and multiple. When the number of figures is small the figures are arranged into one pattern and form a unity. When there are 6, 7, 8 or more figures on the card, they seem to be organized into two (sometimes three) patterns side by side. The figures in the patterns can be simultaneously apprehended and added together in the head at once."

H, [general statement]: "Can often see positions of the figures in memory after-image, and then I count them. Count in groups of 2 or 3." [11 stimulus-objects exposed; "Do not know" reported]: Counted figures of one pattern, jumped to second pattern and then became lost because the patterns overlapped."

The introspections show that our *O*s' reports were based upon two types of apprehension: (1) an immediate type which is divided

[33]The memory after-image was reported by *D* and *H* and suggested by *G*. *D* thought it was a memory rather than a positive after-image because the locus of the image—at the position of the exposure-field—did not change with a shift of his line of regard.

into two sub-types (a) 'unit' or 'field' apprehension, and (b) 'group' apprehension; and (2) a mediated type which is dependent upon re-imaging, counting, and adding.[34]

The limens in this series of experiments are dependent upon the *Os*' range in immediate apprehension and their facility in organizing their impressions in mediated apprehension. Facility in organizing the mediated experience differs in two ways: first in the number of groups apprehended; and secondly, in the number of units apprehended within the single groups. *D*'s and *H*'s range in immediate apprehension was greater than that of *G*, and they were more successful in organizing the mediated material than he. *D*'s range of immediate apprehension was probably no greater than *H*'s, but his facility in organizing the mediated material was greater than hers. He not only habitually apprehended a larger number of groups, but the number of units within his separate groups usually exceeded the number within hers.

The sharp breaks in the *Os*' frequency curves may be explained by the failure of their memory after-images. If we assume that the stimulus-objects can be counted correctly during the clear continuance of the memory after-image and that the count fails when the after-image fades, then the reason for the sharp break is before us. The *Os* fail at the larger stimulus-values not directly because they are large, but only because the large numbers cannot be counted during the short existence of the memory after-image. If, after the exposure of one of the large stimulus-numbers, the memory after-image should last unusually long (a collocation that would not occur very often), or if the stimulus-number were unusually easy to count because of a peculiar arrangement and organization of the impressions (also a collocation that would not occur frequently) we should expect to find that the large number was judged correctly. Our results bear out the expectation; a few straggling correct judgments are given by all the *Os* above the point of their 'break.' The 'point' of the break in the curves of the *Os* can readily be explained by the factors that determined their limens.

Omitting the "I do not know" reports and counting as errors only the judgments that were incorrect, the data show that *G* made the fewest errors, *H* an intermediate number, and *D* the most. The data also show that small errors occured more frequently than large errors, and that negative errors (underestimations) occurred more frequently than positive errors (overestimations). *G* and *H* made no positive errors and their negative errors were never larger than one. *D* made both positive and negative errors of one, and a few negative errors of two.

The number reported in the case of these negative errors may actually have been, as Fernberger has pointed out,[35] the number of stimulus-objects seen by the *Os*. There is that possibility, but as there is no way of deciding the

[34]Except for differences in terminology these results agree with Oberly's findings. Oberly's "immediate" and "grouped" patterns are the same as our sub-types of (1), and his "counted" pattern is the same as our (2). Cf. Dallenbach's criticism of Oberly's terminology, *op. cit.*, this JOURNAL, 36, 1925, 155.

[35]S. W. Fernberger, The 'range of attention' experiment, this JOURNAL, 38, 1927, 478.

point, and as every stimulus-object had the same opportunity of being seen, we had no choice but to count all reports that did not agree with the number of objects exposed as incorrect.

The introspections also show that clearness of experience cannot be correlated with accuracy of report. There are many instances, illustrated by the following excerpts, in which the stimulus-objects were experienced at a high degree of attensity and the reports were nevertheless incorrect:

D, [15 stimulus-objects exposed; 13 reported]: "Impressions equally clear during the exposure."

H, [general statement]: "Sometimes the forms [*i.e.* the stimulus-objects] stand out clearly and distinctly in experience and yet I am unable to report the number exposed."

Converse instances, *i.e.* correct reports when the stimulus-objects were divided in experience between two levels of attensity, occurred occasionally as the following introspections illustrate:

D, [10 stimulus-objects exposed; 10 reported]: "During the exposure the impressions were divided into two levels, the center of the field at the upper level and the margin at the lower. The report of 10 based upon the sum of three groups formed in the memory after-image, upon a group of 5 in the middle of the field, of 3 in the left, and of 2 in the right."

H, [4 stimulus-objects exposed; 4 reported]: "Lower two forms [*i.e.* stimulus-objects] clearer than the two at upper right."

It is clear in view of these findings that experience and report are not unvaryingly correlated: a clear experience is not a guarantee of a full report, nor is a full report a guarantee that experience has been clear.

Experiment III

In the third series of experiments we sought to determine the 'range' at the level of cognition required for naming.

Exposure-cards. Letters were used in this series as the exposure material. They were printed by hand with pen and ink in capitals 1 cm. high. Because of the confusing similarity of C and G, E and F, O and Q, P and R, V and U, and W and M, the second members of these pairs were not used; our exposure material was chosen from the remaining 20 letters of the alphabet. The number of letters printed on the different cards varied from 4 to 9. Twenty cards were prepared for every stimulus-number, giving for the 6 series a total of 120 cards. The letters were printed in two rows in the center of the exposure-cards. When an even number of letters was used, they were arranged in two equal lines; when an odd number was used the extra letter appeared in the first line and the second line was centered under the first. Care was taken in preparing the cards to use the letters equally often, to place them in one position as frequently as in another, and to arrange them so as to avoid words, meaningful syllables, and abbreviations.

The cards were shuffled and given to the *O*s in haphazard order, and the procedure was repeated 5 times; thus 100 reports were obtained for every stimulus-value, or a total of 600 reports from every *O*.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour.

"You will be shown cards which will contain a varying number of black letters on a white background. Immediately after the exposure name the letters that you have seen. Be sure of your judgment; do not guess. Report as many as you can even though you may not be able to name all."

Introspections were asked for at the end of every experimental hour, and they were permitted at any time during the experiments that the *O*s wished to give them.

Results. We give the objective results in Table III, which shows the number of correct reports for every stimulus-value, and the *O*s' limens and measures of precision computed from these data by the method of constant stimuli. We counted a report as correct if the letters were, irrespective of the order of their naming, named correctly. We gave no credit for partial successes; a report was either right or wrong. None of the *O*s named more letters than were exposed. Their errors were errors of omission or naming.

TABLE III

SHOWING THE NUMBER OF TIMES THAT THE *O*S CORRECTLY NAMED THE LETTERS ON THE DIFFERENT EXPOSURE-CARDS, THE LIMENS (L), AND THE MEASURES OF PRECISION (h)

	Number of Letters							
O	4	5	6	7	8	9	L	h
D	98	98	97	74	49	19	7.9	0.43
G	100	100	90	45	11	0	6.9	0.88
H	100	90	33	14	0	0	5.9	0.82

D's limen for naming is the largest among our *O*s (7.9), *G*'s is second in size (6.9), and *H*'s is the smallest (5.9). The rank order of the limens of *G* and *H* is just the reverse of that obtained in Experiment II. *D*'s and *H*'s limens show a marked decline in this experiment, being lower by 3.4 and 3.3 units respectively than in the preceding series. *G*'s limen, on the contrary, is higher in this series by 0.7 of a unit.

The discrepancy between the results of *D* and *H* on the one hand and of *G* on the other is due to *G*'s interpretation of the instructions. "Be sure of your judgment; do not guess" meant very different things for him in the two series of experiments. He says, in one of his introspections of this series of experiments, "I have taken 'guess' in the instruction in the literal sense and not in the sense that I took it in the previous experiment where I regarded it as the

direct opposite of 'absolute certainty.'" He regarded, in other words, every answer that he was not absolutely certain about in Experiment II as a 'guess' and consequently withheld his judgment and reported "I do not know." This introspective evidence is borne out by his objective performance: of the 1300 reports returned by him 4 were incorrect, 372 were correct, and 924 were "I do not know." As a consequence of this strict interpretation, *G*'s limen in Experiment II is not comparable with the limens of the other *O*s nor with his own limen in this experiment. His limen in Experiment II is probably between 2–3 units smaller than it would have been had he not rigorously interpreted the instructions.[36] *G*'s shift from the rigorous to a more liberal interpretation of the instructions may be accounted for, in part at least, by the fact that the report 'I do not know' was not permitted in Experiment III. *G* had to report after every experiment, even though he knew his reports to be incomplete; as a consequence he reported everything about which he was reasonably certain

The introspections given by the *O*s in this experiment show (*1*) that the reports are of the immediate and the mediate types described in Experiment II; (*2*) that attense or clear impressions are not invariably named correctly; and (*3*) that many other factors besides the attensity of the experience condition the reports.

(*1*) The following excerpts from the *O*s introspections show that they gave reports of both the immediate and mediate types.

D: "The names of a few letters may be given immediately, but for the most part the naming is done from the memory after-image."
G: "Sometimes first become aware of a letter when I hear myself reporting it, but there is usually a 'flash' stage before the coming of the letter meanings."
H: "Usually give first line of letters immediately, but the report of last line is based upon the memory after-image of the experience."

(*2*) The fact that there is no invariable relation between the impressions clearly experienced and those correctly named is shown by the following excerpts.

D, [8 letters exposed; 5 correctly named]: "Can not name the rest of them although I am certain that they were as clearly perceived as the ones named." [9 letters exposed; 7 correctly named]: "During the exposure, experience at upper level of clearness, all parts were equally clear."
G, [8 letters exposed; 4 correctly named]: "Am positive that unnamed letters were as clearly perceived during the exposure as those named."
H, [9 letters exposed; 5 correctly named]: "Saw all the letters clearly but lost those of the last line before I could name them."

The converse, that all the impressions correctly named had not been attended to, is not demonstrable in this experiment as it was in Experiment II because the *O*s did not happen in any of their introspections to report two levels of attensity. We have no doubt but that two levels occurred, though

[36] According to the results of Cooper's study, the difference between the limen of absolutely certain judgments (degree of assurance 5) and the limen of judgments that are fairly certain or better (degree of assurance 3, 4, and 5) is approximately 2 units (cf. S. F. Cooper, *op. cit.*, this JOURNAL, 40, 1928, 264-267).

it should be noted that conditions were especially unfavorable for the experience of the exposure-material at two levels because the letters were clustered around the center of the card at the point of fixation, and never more than 9 were exposed at one time.

(3) That the attensity of experience is not the only or even the chief factor determining the report is shown by a comparison of the objective and the introspective results, and by a corroborating report from one of our *Os*:

D, [general statement]: "What I cognize depends not so much upon what is clearly experienced as upon other factors such as order of naming the letters, ease with which associations arise, freedom from inhibition, and so on."

We found, in the examination of our data, that the following factors affected the reports of our *Os* in this experiment: (1) degree of assurance with which the report was given; (2) duration of the memory after-image; (3) order of report; and (4) facility of verbal association.

(1) The effect of assurance can be shown only in the results of *G*. As we mentioned and discussed above, the size of *G*'s limens vary inversely with the degree of certainty of his reports. Similar results concerning the range of information have been obtained in the *Aussage* studies.

(2) As the introspective excerpts in (*1*) above show, the memory after-image plays an important role in the naming of the letters. The longer the duration of this after-image, the more extensive is *O*'s report.

D, [9 letters exposed; 7 correctly named]: "During the exposure, experience at upper level of clearness. . . . First two letters immediately reported, the others were cognized from the memory after-image, which faded before I had named all the letters and so was unable to give the last two."

H, [8 letters exposed; 4 correctly named]: "All were equally clear, could have reported all if report could have been instantaneous. Lost memory images of the last letters before I came to them."

(3) The usual order of report followed by all the *Os* was to name the letters from left to right, and those in the top row before those in the bottom. This plan places the letters at the left at an advantage over those at the right, and those in the upper row at an advantage over those in the lower. This theoretical advantage is borne out by the objective results. Now and again errors and failures may appear elsewhere, but by far the greatest percentage of them occur to the right and in the lower row.

(4) The naming of the letters depends in great part upon the rate and ease with which associations—*i.e.* the letter names—arise.[37] If the associations

[37]Our results corroborate those of Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 233 ff. The illustrative excerpts are drawn from the introspections of *D* and *H*. *G* gave frequent reports but they were in the main devoted to such phenomenological descriptions as the following: "There was a very definite mass of dull, strain-like pressure in the throat. Very dense. When trying to recall other letters there came a strain, a wiry experience, just below the bridge of my nose. Second pressure experience was very slight and a dull sort of thing. Experience in throat was a mass of dull pressury experiences." All of which is excellent phenomenology but none of which is pertinent to the present experiment. The nature of *G*'s introspections did not unfortunately come to the senior author's attention, because he was himself acting as an *O* in the experiment, until after the completion of the work.

come rapidly and freely as many as 7, 8, and 9 letters may be named correctly by *H*, *G*, and *D* respectively before the memory after-image is lost; but if the associations are inhibited, for any one of a number of reasons, and the memory after-image disappears, the processes of cognition are interrupted. In cases of inhibition the *O*s do one of five things.

(a) They give up; their reports are interrupted at the point of the inhibition. Examples are:

D, [9 letters exposed; 1 correctly named]: "Reported H but knew it was B; intended to say B. This error and its correction inhibited cognition of the rest of the letters."

H, [9 letters exposed; 4 correctly named]: "Reversal of position of H and J delayed me to such an extent that the memory after-image disappeared before I could proceed with the naming."

(b) They change their usual order of report. Examples are:

D, [6 letters exposed; 6 correctly named]: "Reported last line first because I was held up by the first letter of the first line." [8 letters exposed; 8 correctly named]: "Intended to report upper line first, but W, the first letter of the last line, protruded and it was involuntarily pronounced, so I reported last line first and then returned to first line."

(c) They carry the meaning of the inhibited item in other terms, and translate it after the formal report. Examples are:

D, [9 letters exposed; 1 correctly named]: "Reported H but knew it was a B." [6 letters exposed; 6 correctly named]: "Called the V a Z although I knew it was a V."

H, [6 letters exposed; 6 reported]: "Said B instead of V for the first letter."

(d) They curtail their reports. Examples are:

D, [7 letters exposed; 5 correctly named]: "Inhibition in first line . . . retarded naming to such an extent that I practically lost lower line."

H, [8 letters exposed; 6 correctly named]: "Lost last two letters because I failed to name X in its correct position after the Z."

(e) Or they skip the letter causing the inhibition. Examples are:

D, [8 letters exposed; 7 correctly named]: "Named first three letters in upper line, skipped the last one because name did not come easily, went to bottom line and named across." [5 letters exposed; 5 correctly named]: "Could not think of S so went to O and then back to S at the end."

H, [6 letters exposed; 5 correctly named]: "Inhibition made me lose the middle letter of last line."

Among the various causes of inhibition the *O*s reported the following.

(1) Transpositions in the order of naming the letters. Not all the transpositions, however, caused inhibitions. Many were made, as the objective records show, that the *O*s were not aware of. Awareness of a transposition appeared to be necessary to cause the report to be inhibited.

D, [9 letters exposed; 3 correctly named]: "Transposed second and third letters; that inhibited the naming of the rest."

H, [9 letters exposed; 4 correctly named]: "Reversal of position of H and J delayed me to such an extent that the memory after-image disappeared before I could proceed with the naming."

(2) The arrest of attention by some aspect in the experiment.

D, [8 letters exposed; 0 reported]: "Second letter in bottom row caught my attention. Though it was the clearest item of experience I could not name it, neither could I name any of the other letters. It inhibited them all."

(3) The feeling of surprise.

D, [8 letters exposed; 4 correctly named]: "Perceived all 8 letters very clearly and felt certain that I could name them. Named top row from left to right. When I turned to name the first letter of bottom row found to my surprise that it was the same letter in cognition as the first letter of the first row. This feeling of surprise inhibited further cognition and I was unable to tell what the rest of the letters were although I am certain that they were as clearly perceived as the letters named."

(4) The misnaming of letters.

D, [8 letters exposed; 4 correctly named]: "Misnamed second letter, corrected it. This inhibited me sufficiently long to interfere with correct cognition of letters toward end of series."

(5) Mental block.

H, [8 letters exposed; 4 correctly named]: "For some reason I could not start my report immediately after the exposure. This delay made me forget the last line."

(6) Stumbling in the pronunciation of a letter.

D, [7 letters exposed; 4 correctly named. S and Z last two letters in upper row]: "Rapid pronounciation of S and Z caused me to stutter—an inhibition sufficiently disturbing to obliterate partially the memory after-image of the letters of the lower row."

(7) Extraneous associations.

H, [6 letters exposed; 6 correctly named]: "Top line A C H. The letters of my high school, A C H S, immediately came to mind. This inhibition nearly made me lose the last line." [5 letters exposed; 5 correctly named]: "The letters of the top line are the initials of a friend; the thought of that almost made it impossible to give the rest of the report."

Experiment IV

The exposure-materials of Experiment III were familiar and a high degree of cognition was not necessary for their report. In the present series of experiments, which is practically a repetition of the third, we sought to induce a higher degree of cognition than that required in the third series by using less familiar exposure-materials. We wished in particular to learn what effect familiarity with the exposure-material has upon the 'range' and upon the conditions affecting the report.

Exposure-cards. The geometrical forms used as the exposure-materials in Experiments I and II were again used in this series of experiments.[38] One hundred exposure-cards, 20 for every stimulus-number from 2–6,[39] were prepared in the manner described above. Care was taken in making the cards to see that the forms were scattered evenly over the exposure-field (as fre-

[38] We used specifically the 15 forms of Experiment II.

[39] We decided upon these limits because we found, in a preliminary series of experiments with *D* in which from 3–9 of these geometrical forms were exposed, that he was unable to name all of the forms on a card correctly when the higher stimulus-values were presented.

quently at the left and right as in the middle, and in the upper and lower parts as in the central), that they were not arranged objectively in patterns, that no single form was used more than once on a given card, and that the different forms were used equally often in the entire series. The cards were shuffled and presented to the *O*s 5 times in haphazard order. A total of 500 reports, 100 for every stimulus-value, was obtained from every *O*.

Instructions. The following instructions were read to the *O*s at the beginning of every experimental hour.

"You will be shown cards which contain a varying number of black forms on a white background. Immediately after the exposure name all the forms you have seen. Be sure of your judgment; do not guess."

As before, introspections were asked for at the end of every experimental hour, and they were permitted at any time during the experiments that the *O*s wished to give them.

Results. We give the objective results for this series of experiments in Table IV, which shows, for every stimulus-value, the number of times that the *O*s correctly name the geometrical form exposed, together with the limens and measures of precision as computed by the method of constant stimuli. As in the previous experiment we counted a report as correct only if the *O*s

TABLE IV

SHOWING THE NUMBER OF TIMES THAT THE *O*S CORRECTLY NAMED THE GEOMETRICAL FORMS OF THE DIFFERFNT EXPOSURE CARDS, THE LIMENS (L), AND THE MEASURE OF PRECISION (h)

	Number of Forms						
O	2	3	4	5	6	L	h
D	100	92	54	28	3	4.3	0.71
G	100	59	18	0	0	3.2	0.81
H	99	89	39	8	0	3.9	0.92

named all the forms on the card exposed correctly. The *O*s' errors were for the most part errors of omission; they never named more forms than appeared on the exposure-card, and they made few errors in naming.

The limens of the number of forms correctly named are 4.3, 3.9, and 3.2 for *D*, *H*, and *G* respectively. The ranking of *G* and *H* is again reversed; it is the same for our *O*s in this experiment as in Experiment II.

The differences in size between the limens of this and the preceding experiment is -3.7 for *G*, -3.6 for *D*, and -2.0 for *H*. These differences must be ascribed to the exposure-materials, for everything else, including the instructions, was kept constant in the two experiments. It is difficult to say exactly what this difference is, but if we may designate it as a difference of 'familiarity'

we may conclude as a result of these findings that the *Os*' reports are, in addition to the factors discovered in the preceding experiments, conditioned also by their familiarity with the exposure-materials.

The introspective data of this experiment give us nothing new; they merely corroborate the findings obtained in Experiment III. They show (1) that the *Os* returned reports of both the immediate and mediate types; (2) that there is no invariable relation between attention and report; and (3) that the completeness of the report is conditioned (a) by the habitual order of naming, (b) by the duration of the memory after-image, (c) by the ease of association, and (d) by the freedom from inhibition—the converse of (c). When the name-associations of the geometrical forms failed, the introspections show that the *Os* resorted to the expedients they noted in Experiment III: they interrupted their reports at the points of inhibition, changed the usual order of naming, carried the meaning of the inhibited item in other terms, curtailed their reports, or skipped the inhibiting item and continued their report.

To the causes of inhibition noted in Experiment III must be added stimulus-material. The introspections show that the name-association is more difficult in the case of geometrical forms than in the case of letters. Inhibitions not only occur more frequently in the experiments with the geometrical forms, but they also, as the objective results show, are more effective in curtailing the reports. If, because of the greater number and effectiveness of these inhibitions, the association-time for geometrical forms is longer than that for letters we should have at once the explanation of our results and the basis of the 'factor of familiarity' mentioned above, for within the limits set by the duration of the memory after-image fewer geometrical forms could be (and actually are) cognized than letters. Lacking experimental results on this point we merely offer this as a possible and plausible explanation.

Experiment V

In the fifth series of experiments we sought to determine the 'range' at the level of cognition required for a description of the exposure-materials. We increased the complexity of the stimulus-objects and changed the instructions to meet the altered conditions.

Exposure-cards. Twelve of the geometrical forms used in Experiment IV were again used in the exposure-cards of this series.[40] Sixty cards were prepared, 20 for every one of the three stimulus-values, 2, 3, and 4, in the manner described above. The forms were concentrated near the center of the exposure-field and care was taken to avoid arranging them into objective patterns. Every form was used an equal number of times in the series. None was used more than once on the same card. After the forms had been punched into the cards, small squares of colored paper were glued over the holes so that the cards from the exposed side showed a number of differently colored forms. Five colors (red, orange, yellow, green, blue) and black were used.[41] These qualities were used equally often in the series, and no quality was used more than once on the same card. The exposure-cards were shuffled and presented in haphazard order 5 times, making a total of 300 observations, 100 for every stimulus value, for every *O*.

TABLE V

SHOWING THE NUMBER OF TIMES THAT THE *Os* CORRECTLY DESCRIBED THE DIFFERENT EXPOSURE-CARDS, THE LIMENS (L), AND THE MEASURES OF PRECISION (h)

	Number of colored forms				
O	2	3	4	L	h
D	64	52	17	3.3	0.45
G	88	59	4	3.0	0.97
H	92	67	18	3.3	0.83

Instructions. The following instructions were read to the *Os* at the beginning of every experimental hour.

"You will be shown cards which contain a varying number of colored forms on a white background. Immediately after the exposure describe the forms you have seen, that is, give the number, names, and qualities of the stimulus-objects. Be sure of your descriptions; do not guess."

At the end of every experimental hour the *Os* were asked, as in the other experiments, to give an introspective report. They were also permitted as before to give introspections in the course of the experiments at any time they wished.

Results. We give the objective results in Table V, which shows the number of times that the *Os* correctly described the exposure-cards, and the limens and measures of precision computed from those data by the method of constant stimuli. We counted a description as correct if the *Os* named all the forms and

[40]Dot, disk, 'Saturn,' globe, semi-disk, angle, triangle, square, heart, spade, Greek cross, and diamond. The club, rectangle, and 'crown' were omitted from the previous series.

[41]We used specifically the following colors: the 'Farbenkreis Rot,' *i.e.* the 'Hering Red,' Stoelting color no. 1; the Milton-Bradley 'Orange;' the 'Farbenkreis Gelb,' *i.e.* the 'Hering Yellow,' Stoelting color no. 5; the Milton-Bradley 'Green;' the 'Farbenkreis Blau,' *i.e.* the 'Hering Blue,' Stoelting color no 13. Nothing was pasted over the holes we wished to appear 'black.' The surface of the card-holder sufficed for that quality.

their respective qualities correctly. As in the other experiments, we did not credit partial successes: a report was either right or wrong. If, as frequently occurred, the *O*s named the forms and qualities correctly but failed to correlate them correctly, we counted the report as wrong.

The 'descriptive' limens are 3.3, 3.3, and 3.0 for *D*, *H*, and *G* respectively. *H* and *D* are tied, and *G* is but slightly below them. The limens in this series of experiments are smaller than those of Experiment IV by 1.0 for *D*, 0.6 for *H*, and 0.3 for *G*. These differences, except perhaps the difference for *D* (which is, as we show below, enlarged by our arbitrary treatment of the data), are surprisingly small; we had expected that the limens for naming the 'form and quality' would be much smaller than for naming the 'form' alone. The differences, however, though small are constant and, due to the large number of cases involved in the two experiments, significant. We feel justified, therefore, in concluding that degree of cognition required by the instructions is another factor in determining the *O*s' reports.

In addition to the errors of correlation mentioned above, the *O*s' errors were for the most part errors of omission and qualitative description. Very few of the geometrical forms were reported incorrectly and no errors of addition of either forms or colors were made. The *O*s showed striking individual differences, however, in the kind of errors that they made: 79% and 73% respectively of *G*'s and *H*'s errors were errors of omission, whereas only 9% of *D*'s errors were of that kind. *D*'s errors were principally errors of correlation and of color naming. The difference between *G* and *H* on the one hand and *D* on the other may be due in part to a difference in their method of reporting. *G* and *H* gave the quality with the form as "red square, green diamond, black triangle," but *D* seldom gave his reports in that fashion. He named the forms first and then the colors, or contrariwise the colors first and then the forms. He adopted this method of reporting, as the following excerpt from one of his introspections shows, because he found that the transition from one aspect of the report to the other inhibited his associations more than if he completed his report of one aspect before taking up the other. He says:

"The name associations are not inhibited so much if I name the colors and then the forms, or vice versa, but when I transfer from one aspect to the other the report is difficult and halting. I do not consciously, however, set myself to report one or the other aspect first."

What he did in a particular case was determined, as the following excerpts show, by the freedom of his associations; he reported the aspect which was not inhibited, or, if neither were inhibited, the one which seemed to be the more fleeting and insecure.

"Held up by the first color, so reported the forms first and then returned to the qualities." "Named the colors first because the forms seemed more secure."

The possibility of correlational errors, the kind that appeared the most frequently in *D*'s data, would be much greater with *D*'s method of report than with the method of the other *O*s, but we do not, however, believe that the method of reporting is the chief cause of the errors made by *D*; it is at most nothing more than a contributing factor. The principal cause of *D*'s errors and the large decline in the size of his limen in this experiment is, we believe, his defective color-vision. *D* is partially color-blind, of the deuteranopic type.[42] He omitted the green forms more frequently in his reports than those of any other quality, and when he did report it he most frequently cognized it as red or orange, and now and then as blue. To count such reports as errors is to disregard the nature of *D*'s color-perceptions. We had no other choice, however, for the objective standard was the only check that we had upon the *O*s' reports. We could not presume to differentiate between perceptual and cognitive errors, and there was no need for the purpose of the present experiment for us to do so. These data clearly indicate the point, without laboring it further, that report is conditioned by color-sensitivity as well as by the other factors thus far mentioned.

The introspective data of this experiment, in addition to corroborating the results of the preceding sections, show that the cognition of form and of color are not of equal difficulty. *G* and *H*, as the following excerpts show, find that the cognition of form is more difficult than the cognition of color.

G, [general statements]: "Harder to apprehend form." "Colors apprehended at the time of the exposure, not true of the forms."

H, [general statements]: "Forms are very difficult." "Often find it hard to give the name of the forms."

These reports of the *O*s are borne out by their objective results: 46.3% of *G*'s 149 incorrect reports are due to errors (omissions and misnamings) in form, 45.0% to errors in both form and color, and 8.7% to errors in color; 42.3% of *H*'s 123 incorrect

[42]For a further description of *D*'s anomalies of color cognition see Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 219.

reports are due to errors in form, 43.1% to errors in both form and color, and 14.6% to errors in color. *D*'s results are just the opposite. He finds, as the following excerpts show, that the cognition of color, probably because of his defective color-vision, is more difficult than the cognition of form.

D, [general statements]: "Names of the colors come with great difficulty." "Association of the color names is often inhibited."

His objective results bear out his statement also: 3% of his 167 incorrect reports are due to errors in form, 15% to errors in both form and color, and 82% to errors in color. We have here further confirmatory evidence that the reports are affected by the kind of stimulus-materials exposed and by the color sensitivity of the *O*s.

Summary and Discussion of Results

We carried a group of 3 *O*s through 5 series of tachistoscopic experiments in which we attempted to duplicate under standard and comparable conditions the outstanding experiments that have been conducted upon the range of attention. Since we used the same *O*s throughout the study, the results of the different series may be directly compared. The experiments were of two kinds: those (Experiment I) in which we sought to escape the cognitive complications of report; and those (Experiments II-V) in which we took cognition into account and attempted to control and to vary it by the instructions and the kinds of exposure-materials used.

We found as the result of these experiments: (1) That the number of stimulus-objects exposed is not a compulsory condition of the two-level experience of the exposure field.

Number was effective for only one of our *O*s (*D*), and it was not even a contributing condition for the other two (*G* and *H*). *G*'s results show that he was 'set' for a uniform clear experience of the exposure-field; *H*'s results show that her experiences of the exposure-field (since she reported the one- and two-levels approximately equally often for every stimulus-value) were determined by chance subjective conditions—'chance' because we did not know that they were and had no control of them, and 'subjective' because the objective conditions were controlled and were constant, except for the number of stimulus-objects exposed, which was not effective in her case. *D*'s data indicate that his reports also, since his results for the different Form-Series deviated widely, were conditioned by subjective factors. In his case, however, the subjective conditions were not sufficiently strong or numerous to mask the effect of the objective factor, the number of stimulus-objects exposed.

(2) That, in the 'cognitive' series of experiments, the range, defined as that value of stimulus which has a 50% probability of being correctly reported, varies (a) inversely with the degree of cognition required for the report;

We used the same exposure materials (geometrical forms) in Experiments II, IV, and V, with different instructions. We asked the *O*s in Experiment II to report the number of stimulus-objects exposed, in Experiment IV to name them, and in Experiment V to describe them. The 'ranges' in these three experiments were 11.3, 4.3, and 3.3 respectively for *D*; 9.2, 3.9, and 3.3 respectively for *H*; and 6.2, 3.2, and 3.0 respectively for *G*—thus clearly establishing the fact that the higher the degree of cognition the smaller is the range of report, and contrariwise, the lower the degree of cognition, the higher is the range of report.

(b) with the kind of material exposed;

We used the same instructions (to name) in Experiments III and IV, but different exposure materials. In Experiment III we used letters, and in experiment IV geometrical forms. The 'ranges' in these two experiments were 7.9 and 4.3 respectively, for *D*, 6.9 and 3.2 respectively for *G*, and 5.9 and 3.9 respectively for *H*.

(c) inversely with the degree of assurance or certainty of the *O*'s report;

The effect of the degree of certainty or assurance upon the report is demonstrated in the results of but one *O* (*G*), and by him only accidentally. *G* rigidly interpreted the instruction "do not guess" in one series of experiments (Experiment II) to mean 'do not report unless absolutely certain;' in another series of experiments (Experiment III) he took 'guess' in its "literal sense," and reported everything about which he was reasonably certain. In the series in which he was 'absolutely certain' of his reports his range was 6.2; in the series in which he was 'reasonably certain' his range was 6.9. The effect of the differences in certainty is greater than these results show, for these results are cut across and partially obscured by the differences in the degree of cognition involved in the reports. The experiments involving the lower degree of cognition were reported under the limitations of the highest degree of assurance, those involving the higher degree of cognition were reported at a lower degree of assurance. The fact that an increase in the degree of certainty showed its effect, under these circumstances, by a decrease in the 'range' indicates that for *G* at least the degree of assurance required for a report is more effective in determining the range of that report than the degree of cognition.

(d) with the duration of the memory after-image;

The introspections of all the *O*s show that the memory after-image plays an important rôle in all of the cognitive experiments. The longer the duration of the memory after-image the more extensive is the report, and the greater the degree of cognition required for the report the more necessary is the after-image to the report.

(e) with the facility of verbal association and the freedom from associative inhibitions;

Both the introspective and objective results show that the greater the degree of cognition required for the report, and the shorter the duration of the memory after-image, the more important are the verbal associations and the more injurious to the report are the associative inhibitions. The *Os* mentioned the following causes of inhibitions in their introspections: transpositions of the usual or intended order of report, distractions, feelings, misnamings, stuttering, mental block, and extraneous associations. The list is by no means exhaustive, the *Os* were not 'set' to report the causes of their inhibitions. The causes listed are only those that were incidentally reported. When inhibitions occurred the *Os* either gave up, changed their usual order of report, carried the meaning of the inhibited association in other terms, curtailed their reports, or skipped the inhibited item and continued the report.

and (f) with the color sensitivity of the *Os*.

One of our *Os* (*D*), is partially color-blind. In Experiment V, in which reports of color were required, 97% of his incorrect reports were due to errors involving color.

(3) That the *Os*' reports are based upon two types of apprehension: (1) an immediate type which is divided into two subtypes: (a) 'unit' apprehension, and (b) 'group' apprehension; and (2) a mediate type which is dependent upon re-imaging and other ancillary processes.

When a low degree of cognition is required (as in the cognition of number in Experiment II), or when the stimulus-material is relatively simple (as the letters in Experiment III) and few in number, the *Os*' reports are based upon what we have chosen to call 'unit' apprehension. The impressions are apprehended as separate units and the reports are immediately touched off by the 'given' without the mediation of other conscious processes. When more stimulus objects are exposed or the stimulus conditions are a little more complex, the *Os*' reports are based upon 'group' apprehension. The impressions are apprehended as a group and the reports are immediately made as before without mediating experiences. When the higher degrees of cognition are required, or more complex stimulus-materials are used, the *Os*' reports are mediated by various experiences.

(4) That large individual differences in the range of cognition exist among our *Os*.

(5) That the individual differences vary inversely in size with the degree of cognition required for report.

The greatest differences in range among our *Os* occur in the experiment (Experiment II) requiring the lowest degree of cognition, and the smallest differences occur in the experiment (Experiment V) requiring the highest

degree of cognition. The maximal difference among the *O*s at every degree of cognition is, arranged from the highest to the lowest degree of cognition, 0.3, 1.1, 2.0, and 5.1.

(6) That the clearness or attensity of experience is not correlated with fullness or accuracy of report.

Introspections from all the experiments show many instances in which the impressions of the stimulus-objects were experienced at a high degree of attensity and the reports of them were incorrect. Instances of the converse, impressions correctly reported that were experienced at a low degree of attensity, were obtained only in Experiment II, for it was only in that experiment that the *O*s reported their experiences of the exposure-cards at two levels of attensity. All the errors of report, particularly in Experiments IV and V, may be regarded as instances in which the impressions attended to were not reported.

Conclusions

Because of the facts here reviewed, in particular because the number of stimulus-objects exposed is not a compulsory condition of the two-level experience, because cognition and report are conditioned by many other factors besides attention, and because the attensity of experience is not correlated with the accuracy of report, we feel justified in concluding that range is not a proper question to set to attention. The attentive consciousness is an integrated whole and as such the range is always 'one.' Questions concerning the number of contents or part contents that may be simultaneously experienced are questions, since cognition is nherent in report, that concern cognition and not attention.

A NEW CLASSIFICATION OF THE RED-GREEN COLOR-BLIND[1]

By Sibyl Walcutt Terman, Stanford University

I. Introduction and Historical Summary

While a great deal of work has been done on the subject of red-green color-blindness, we have still no very definite idea of exactly what the color sensations of the color-blind are, nor have we any test which will give us the facts for any given individual. We know that to a severe case the spectrum appears blue and yellow, with bands of gray, but most workers grant that there are many grades of the defect existing in different subjects; yet few have attempted a description of the variations in color sensation which must exist among them. Some of the best known attempts in this direction consist for the most part in dividing subjects into broad categories only. They may be briefly summarized as follows:

(*A*) *Protanopes and deuteranopes.* These terms were first suggested by von Kries[2] to mark off the two types of color-blindness which are known to exist, and which are accepted even by the advocates of conflicting color theories. The deuteranopes, or green-blind, are those whose color system is reduced to yellow and blue, but who see color throughout the spectrum; the protanopes, or red-blind, are those whose color system is reduced to blue and yellow, but whose spectrum is shortened at the red end. That is, they see red as black. With them the point of maximum brightness is shifted from the yellow to the green. This differentiation is commonly accepted today. The advocates of the Young-Helmholtz,[3] the Hering, the Ladd-Franklin and other theories of color vision all accept this classification.

(*B*) *Edridge-Green's classification.* Edridge-Green,[4] the inventor of the lantern for testing color-blindness, has a quite different view. He divides the

*Accepted for publication October 21, 1928.

[1]This work was undertaken at the suggestion and carried out under the guidance of Professor W. R. Miles. The expenses connected with the investigation were largely met from the Thomas Walton Stanford Fund for Psychological Research.

[2]M. Collins, *Colour-Blindness*, 1925, 12.

[3]M. Collins, *op. cit.*, 33, 44, 51.

[4]F. W. Edridge-Green, *Physiology of Vision with Special Reference to Colour-Blindness*, 1920, 193-205.

color-blind into 7 groups which are supposed to correspond to seven stages in the evolution of the color sense in the race. To the first group the entire spectrum appears gray. The second group sees a tinge of red at one end of the spectrum and of blue at the other. For the next group the colored sections invade the gray until they meet in the center, and are seen as blue and yellow. Next a third color, green, appears between the two. Such cases see red, green, and violet, but not blue and yellow. The next group see red, yellow, green and violet, and the sixth see red, yellow, green, blue and violet. Orange is the sixth color to be recognized. This is the normal group. The highest development yet reached is that of the group who can distinguish indigo in addition to the other colors. This is the sort of classification and description which we need. Unfortunately, few, if any, agree with this grouping.

(*C*) *Ishihara's classification.* Ishihara,[5] Japanese inventor of the test described later, accepts the red- and green-blindness classification, but finds that some cases of red-green blindness are accompanied by a weakness in the blue-yellow sense, while some are not. The former type of defect he designates as "incomplete total color-blindness," that is, a weakness for all colors.

(*D*) *Collins's results.* In a recent book, Collins[6] makes a study of ten cases of red-green blindness, in an attempt to find out more exactly, uninfluenced by any color theory, just what the color experiences of the color-blind really are. Most of the standard tests for color-blindness, as well as equations on the color wheel, contrast experiments, etc., were used.

Collins gives a detailed statement of the confusions made by each *S*. She found that the deuteranope and protanope classification held good, but was inadequate for a complete description, as each case was unique and each represented a different stage of defective vision. One *S* apparently saw a small band of blue on one side of the spectrum and a corresponding one of yellow on the other side with only two broad gray bands between, while others could actually see red and green under certain conditions. The final conclusion of Collins is that "it seems unprofitable to systematize the color sensations of the color-blind; so many individual types exist."[7]

(*E*) *The four-stage classification.* The author wishes to offer, as a result of her work with 60 color-blind *S*s, a new explanation of what the color-blind see, which takes account of the quantitative and qualitative differences found, and which offers a practical scoring method.

Most work in color-blindness has been done either with a few colors commonly confused by the color-blind, or with a great hodge-podge of all sorts of unstandardized colors, or with the hues of the spectrum in carefully standardized wave-lengths. All of these methods were felt to be inadequate. The color unit which is psychological in character is the color sphere, and a test which aims to find out the facts of color vision must take all three dimensions

[5] S. Ishihara, *Tests for Colour-Blindness*, 5th ed., 1925.
[6] *Op. cit.*, 189 ff.
[7] Collins, *op. cit.*, 210.

of the sphere—hue, saturation, and value—into account. By using a color-sorting test in which the colors were chosen to represent the sphere as completely as possible, the four-stage classification of the color-blind was arrived at. It may be described as follows.

If we had some way of finding out exactly what the color sphere looks like to each of our *S*s, and could make an objective representation of the spheres as they appear to each separate individual, we should find that these spheres represented a gradually descending scale of color vision from the almost perfect to the nearly monochromatic. Although we had none among our *S*s, there are individuals reported who see every hue of the sphere as neutral gray. If we tried, however, to arrange these color spheres, or objective representatives of what each *S* saw, in an orderly series such that the best was at the top and the poorest or most nearly colorless at the bottom of the scale, and such that the series varied only quantitatively (in amount and number of colors seen) and not qualitatively (some seeing a different assortment of colors than others), we should fail, because there is every variety of individual difference in color vision. But if, instead of using the whole color sphere, we should confine ourselves to a single hue, we would have no difficulty in forming such a series. The representations of what each individual saw, for instance, when he looked at the red cards of a test which are colored to represent many different shades, tints, and saturations of the red hue, could be readily arranged in a series such that at one end there would be the true red as seen by the normal eye, and at the other end the pure gray or black, as red appears to the worst cases. Furthermore, if such a series were made, it would probably be found that the color vision of the many *S*s for this one hue could be roughly divided into 4 classes. Those who saw red normally would be in stage 1, those who saw it as gray, in stage 4, and there would be at least two intermediary stages. Each hue of the spectrum could be classified for each *S* as being in one of these 4 stages. This was our notion as first formulated. As a matter of fact it does not hold in this form because, besides the quantitative difference described, there is also a qualitative one. As actually found in practice the facts seem to be as follows.

Stage 1. The hue is seen normally in Stage 1.

Stage 2. The bright, clear, saturated colors of the hue are seen in Stage 2 in their true hue, though probably not as clearly as by the normal eye. The darker shades, lighter tints, and weaker saturations are seen as either blue or yellow, that is, as the central unit color of the side of the color ring on which they lie. Hues from red to green are seen as yellow, others as blue. In some cases, however, the hue does not change to blue or yellow, but merely becomes a weaker saturation of the true hue.

Stage 3. The bright saturated colors are seen in Stage 3 either as yellow or blue, according to their position in the spectrum; while the light, dark and unsaturated colors are seen as various shades of gray. Here again there are some cases where the bright colors are merely weaker saturations of the true hue and do not change to blue and yellow.

Stage 4. The whole hue is seen in Stage 4 as gray.

There are some exceptions. Occasionally blue-green is seen as yellow and purple as red. This may possibly represent incipient blue-yellow blindness.

By giving an individual a test of sorting, matching and naming colors, and analyzing his mistakes, it is possible to classify each hue of his spectrum or color ring into one of these stages. It is self-evident, of course, that these stages are not so clearly defined and distinct from each other in actuality as this description suggests. They represents rather an unbroken series of color vision gradations, and division into stages is more or less arbitrary.

II. Method and Apparatus

The *Ss* for this research were university and high school students selected by means of the Stilling[8] and Ishihara[9] tests. The former is too well known to need comment. The latter, a test on the same order as Stilling's, is made up of charts on which numbers composed of dots of one color are printed on a background of dots of a contrasting color, but of the same brightness values. Colors chosen are those commonly confused by the color-blind, who fail to read the numbers which are clear to the normal eye. In order to get *Ss* with every grade of defect, some who misread only one or two cards were included as well as those who failed badly. Ten *Ss* who rated 100% perfect on the Stilling and Ishihara tests were used for comparison as controls.

Color-Sorting Test. As stated above, it was felt that a test in this field must be based on the color sphere. For convenience a color ring was used.[10]

[8] J. Stilling, *Pseudo-isochromatische Tafeln zur Prüfung des Farbensinnes*, ed. by E. Hertel, 17th ed., 1926.

[9] *Op. cit.*

[10] Originated by Newton, perfected by Donders and Collins. See F. C. Donders, *Annales d'Oculistique*, 34, 1880, 212; Collins, *op. cit.*, 209.

This ring consists of the hues of the spectrum arranged in a ring, with extra-spectral purple between the red and violet. But the space for each hue was considered to represent, not the spectral color alone, but all the different saturations, tints and shades of the sphere which are included under that hue.

An examination of most of the available color-blindness tests showed that these were all founded on the following methods.[11] Colors are exposed, and the *S* is required to (a) name them, (b) sort into groups of the same hue or match a sample for hue, (c) find an exact match for a sample color, or (d) distinguish between contrasting colors with or without naming them, as in the Stilling or Nagel Tests. In fact, the truth seemed to be that the difference in the tests was mostly one of the source of the color or light, with the exception of the number tests described above, which are of a really distinctive nature.

The color-sorting test used in this experiment was planned with the above principles in mind. It was composed of 95 cards of different colors, to be named and compared with 10 sample cards. Colored papers put out by the Bradley and Munsell Companies in yellow, orange, red, green, blue-green, blue, violet, and purple were used. Of each hue the following values and saturations were available.

Saturated	*Unsaturated*
Bradley Tint II	Munsell light
Bradley Tint I	
Munsell maximum	Munsell middle
Bradley Shade I	
Bradley Shade II	Munsell dark

In addition, a number of cards were painted for the purple, blue-green, red, and green series of both the saturated and unsaturated types, to give a greater variety in those hues which have proven difficult for the color-blind. Taken with the papers, they formed a complete series for each hue, extending from very pale to almost black. A brown series, using a dark orange as a standard, and a gray series were also painted. Each card was plainly numbered.

Besides the above, 10 sample colors were chosen, one to represent each of the 10 hues, and these were arranged on 3 display sheets. The samples were chosen in accordance with Collins's[12] comments on the Holmgren Worsted Test. She found that standard reds and greens made the test too easy; therefore paler or darker and less saturated colors were used. This was later felt to be a mistake.

Procedure. The *Ss* were placed 10 to 12 ft. from the examiner so that differences between painted and paper cards could not be noticed. Every *S* was given 4 mimeographed sheets containing 95 numbered squares. Display sheet No. 1 was placed before the *Ss* and the 95 cards were exposed one at a time for about 2 secs. each. The cards were exposed in chance order so far as hue was concerned, but they were arranged so that the same hue did not appear twice in succession and so that the sequence of hues was different each time. The lightest values were exposed first, then the next darker, etc. The *Ss* compared each card with the samples, and if it seemed to be of the same hue as one of

[11] A. E. Chace, Color-blindness in industry, *J. Amer. Med. Ass.*, 87, 1926.
[12] *Op. cit.*, 71-72.

them they wrote the number of the sample it resembled in the square containing the number of the card. When the pack had been worked through in this way, display sheet No. 2 was used in place of No. 1 and the performance was repeated. The whole procedure was repeated a third time with display sheet No. 3. The pack was then exposed a fourth time while the *S*s named the color on each card, using a list of abbreviations printed on a card placed before them. The whole proceeding took about 40 minutes.

III. Results

(*a*) *The Four-Stage classification of the color-blind.* The results for each *S* on the color-sorting test were collected on a separate sheet, a sample of which is given as Table I.

The numbers in boldface in Table I are those of the 95 color-cards. The captions give the true color names of every group together with the number of the sample card under which the group should have been classified. Opposite the boldface numbers appears the name the *S* gave the exposed color-card and the number of the sample card under which he classified it. A dash(—) indicates that the card was left unclassified at that exposure. An asterisk (*) before a number indicates that it is identical with the sample card. The numbers in italic are the standard ones giving the saturated hue. While Table I will not mean much at first to the reader, for those familiar with the meaning of every number and abbreviation it shows the *S*'s confusions at a glance.

In working over the papers the Four-Stage classification outlined above was formed, at first tentatively, and was tested by drawing up a list of rules and scoring in the light of them each *S*'s color vision for each hue. The rules, given below, are either self-evident propositions or are based on some of the most commonly accepted facts about color-blindness, such as the fact that these people see yellow and blue better than they do other hues.

(1) If all the cards of a certain hue are named and classified correctly throughout, it is taken as evidence that the *S* sees that color as the normal person does, and he is given a rating of '1' in that hue.

(2) The mistakes commonly made by the control *S*s were tabulated and of course were not counted as having any significance.

(3) If a card is called gray or is classified under the gray sample it is evident that it is seen as gray. (See page 213 of Collins.) There were exceptions, however.

(4) If two or more hues are named or classified alike they appear as the same color.

(5) To confuse colors which lay adjacent to each other on the color ring was not counted as a significant mistake. The controls frequently made this error.

(6) If an *S* confuses two colors which both lie on the same side of the color ring, he sees them both as the central color of that side, that is, as either blue or yellow, unless further evidence proves that they are seen as gray.

(7) If two colors are confused which lie on opposite halves of the ring, they are both seen as neutral gray, unless the two colors are adjacent to each

other. Thus red and purple may be confused and still be seen normally, because they are neighbors, but if green and purple are confused, both are seen as gray.

(8) If a wide variety of color names is given in a perfectly random fashion to the cards of one hue, that hue is seen as a neutral gray.

(9) If the gray cards are generally called by a color name, there is a neutral band in that color.

(10) If colors are called blue, yellow, or gray, they are probably seen as such, unless it can be shown that the *S* has a neutral band in the blue, because these are the hues commonly seen correctly by the color-blind.

TABLE I

THE RESULTS OF *S* 34, COLLECTED AND ARRANGED FOR GRADING

The numbers in boldface are those of the color-cards. The captions give the color of every group and the number of the sample card under which every member of the group should have been classified. Opposite the boldface numbers appear the names given the exposed color cards and the numbers of the sample card under which each was classified. A dash (—) indicates that the card was left unclassified; an asterisk (*) indicates that it is identical with the sample card; and the boldface italic indicates the cards of standard hue.

g = green, gr = gray, b = blue, bk = black.

Brown 1	*Red 7*	*Orange 3*
6 g — — 10	**12** gr — — 10	**8** — — — —	**17** y 4 — —
33 br 1 — 10	**39** gr — — 10	**20** gr 2 — —	**35** y 3 — —
46 br 1 7 10	**48** g 1 — 10	**25** — 1 — —	***44*** y 4 — —
61 br 1 7 —	**55** g 2 7 —	**31** gr 1 — 10	**77** o 3 7 —
75 br 1 7 —	***59*** — — 7 10	**49** — 1 — —	***80*** g 1 7 10
	72 br 1 7 10	**66** gr — — 10	**3** y — — —
	89 g 1 7 10	**87** g 1 7 10	**63** g 1 7 —
	*93 g 1 7 10		*83 g 2 7 10

Yellow 4	*Green 10*	*Gray 2*
4 y 4 — —	**19** gr — — —	**26** br — — —	**2** w — — —
16 y 4 — —	*32 — — — 10	**47** g 1 — 10	**15** b — 6 9
****36*** y 4 — —	**43** — — — —	**69** g 2 6 10	**37** r 2 6 8
64 y 3 — —	**54** yg 2 7 10	**73** — 1 7 10	**41** pk 5 6 8
40 r 3 — —	***60*** gr — — 10	**85** — 2 — 10	*62 pk 2 — 8
78 o — — —	***67*** g 1 7 10	**94** g — 7 —	**79** r 2 6 8
81 g 1 7 10	**7** gr — — —		**82** b — — —
	13 b 4 — —		**95** bk — — —

Blue-Green 8	*Blue 9*	*Violet 6*	*Purple 5*
21 pk — — 8	**22** b — — 9	**23** b — — 9	**1** bgr — — 10
****74*** b — 5 8	**51** b — — 9	**28** b — — 9	**5** p — 5 —
58 pk 2 6 8	*57 b — — 9	**52** b — — 9	**53** b — — —
70 pk 2 — 8	**71** b — — 9	***68*** b — — 9	**65** b — — 9
9 b — — 9	***90*** bk — — 9	**11** b — 5 6	***76*** v — 5 9
14 v 2 6 8	**10** v 5 6 9	*56 pk — 8 —	**18** b — 6 9
24 r — 6 8	**29** v 5 — 8	**91** v 2 5 8	**27** p 2 5 6
30 pk 2 6 8	**92** pk 5 — 8		**34** p 2 — 8
38 v — 5 —			**42** p — 6 8
50 r 2 6 8			*45 p — 6 8
88 b — — 9			**84** b — — —
			86 b — — —

(11) If the cards of two hues are both classified under the the same two or three sample cards consistently, they are seen alike, though they may be named differently. When the inconsistency is pointed out, the *S* will say in an aggrieved tone, "Well, I can only tell you how it looks to me."

(12) If both the light blues and the light violets are called blue, and the dark members of the two series are called violet, they are both seen as blue. The explanation is that they are being judged on the basis of brightness and not hue. So for any other hues which are treated in the same way.

(13) The fact that an *S* has classified all the cards of a given hue correctly does not count in his favor, if he has in addition made many other false classifications of them.

(14) The fact that a card is not classified under the blue or yellow is not a certain proof that it is not seen as one of these two colors. An *S* may classify each of the blue-green, violet, and purple cards under all the 3 sample cards of those hues; according to rule (5) above, they are all seen as blue. Yet they may not be included in the cards classified under the blue sample card, and all the blue cards may be correctly classified and named. The explanation is that they are seen as blue, but as a "different color blue," as the *S*s describe it. They appear as a "dead-looking" blue because the red and green in them appear as gray, and they may thus be readily distinguished from the clear bright blues. This is further proven by the fact that the unsaturated and dark blues may be classified with the blue-green, violet, and purple series, rather than with the blue cards.

(15) A color may be named correctly and classified incorrectly. If this is done systematically, the color is seen as classified rather than as named.

While this method of grading the papers is not purely objective and requires a certain amount of 'clinical sense,' it was not found to be as difficult as one would imagine to assign to each *S* a rating for each hue of his color ring which was felt to represent the facts pretty clearly.

Table II gives the ratings according to the Four-Stage classification. It will be seen that in some cases the ratings of 2.5 or 3.5 were used. This was done when the fairest rating seemed to be between 2 and 3, or 3 and 4.

Unexpected confirmation of the theory suggested was offered by one of the *S*s. He claimed to have had normal vision up until four years ago when he had scarlet fever which left him with a curious defect. In foveal vision he exhibits all the ear marks of a typical case of dichromatism; in peripheral vision he has no difficulty in distinguishing red and green. An examination of his eyes with the perimeter showed in each a ring which was sensitive to these colors, but which enclosed an area covering the fovea which was sensitive only to blue and yellow. Here we had an individual capable of describing the sensations of the color-blind in terms of normal vision. His results on the test bear striking witness to the truth of the theory advanced above.

(*b*) *Explanation of types of confusions made.* One of the most puzzling facts brought to light by the color-sorting test was that such different types of confusions were made by different *S*s. Of two *S*s apparently equally color-blind according to the Still-

TABLE II

RATINGS RECEIVED BY THE COLOR-BLIND ACCORDING TO THE FOUR-STAGE CLASSIFICATION ARRANGED ACCORDING TO RANK ORDER OF SCORES

S	Yellow				Blue				Total Score
	R	O	Y	G	BG	B	V	P	
1	1	1	1	1	1	1	2	1	9
2	1	1	1	2	1	1	1	1	9
3	1	1	1	2	1	1	1	1	9
4	1	1	1	2	1	1	1	2	10
5	1	1	1	2	gr2	1	2	1	11
6	1	1	1	2gr	gr3	1	1	1	11
7	1	1	1	2	gr2	1	2	1	11
8	gr2	1	1	2	gr2	1	1	1	11
9	1	1	1	2	y2	2	1	1	11
10	3	1	1	1	gr1	3	1	1	12
11	2	1	2	2	1	1	2	2	13
12	gr2	1	1	2	1	2	2	2	13
13	gr2	1	1	3·5	gr3·5	1	1	1	14
14	2	1	1	3·5	gr3·5	1	1	1	14
15	2	2	1	3gr	gr2	1	2	2	15
16	3	2	2	2gr	1	1	2	2	15
17	2	1	1	2gr	4	1	2	2	15
18	2	2	1	2	gr3	2	2	1	15
19	2	2	1	3	gr3	1	2	2	16
20	2	1	1	3	y3	2	2	r2	16
21	1	2	1	3	4	2	2	1	16
22	2	2	1	2gr	4	2	2	2	17
23	3	1	1	4	4	1	2	1	17
24	2·5	1	1	4	4	1	2·5	2	18
25	2	3	1	3	y3	1	3	r2	18
26	2	2	1	3	4	1	2	4	19
27	2	2	2	3	y3	2	3	2	19
28	gr2	2	1	3	4	1	3	3gr	19
29	3	1	2	3	4	1	3	3	20
30	4	1	1	3	4	1	3	3	20
31	2·5	2	2	2·5	4	3	2	2	20
32	3	2	1	3	b3	2	3	3	20
33	3	3	1	3	4	1	3	3	21
34	3	2	2	3	4	2	2·5	3	21·5
35	4	2	1	3·5	4	2	2	3	21·5
36	2	3	1	3	4	3	3	3	22
37	3	3	1	3	4	3	2	3	22
38	4	2	2	3·5	4	2	2	3	22·5
39	3·5	2	2	3·5	gr3	2	3	4	23
40	3·5	2·5	1	3·5	4	2	3	3·5	23
41	3	2	2	4	4	3	3	2	23
42	3	2	2	4	4	3	3	r2	23
43	4	3	2	3	4	3	3	2	24
44	4	3	1	3	4	3	3	3	24
45	3·5	3	1	4	4	2	3	4	24·5
46	4	3	2	4	4	2	3	3	25
47	3·5	3	3	4	4	2	3	4	26·5
48	3·5	3	1	4	4	3·5	3·5	4	26·5
49	4	4	2	3	4	3	3	4	27
50	4	4	2	4	4	4	2	4	28
51	4	3	1	3	4	4	4	3	30

ing Tables, one would consistently confuse the red and green cards, while the other would not make one such confusion. On the other hand, the latter would confuse red with brown and yellow, and green with gray, so consistently that one could well doubt whether he saw either red or green at all. In other cases *Ss* confused the unsaturated reds and greens, but saw the bright ones well enough to identify them correctly.

The explanation of these and similar phenomena becomes clear in the light of the Four-Stage classification of the color-blind. It is self-evident that those cards which looked alike were confused while those which appeared different were distinguished from each other.

(1) If both red and green are in Stage 1, no confusion can take place.

(2) If both hues are in Stage 2, the brightest colors which have the normal hues will not be confused, but the pale, dark and unsaturated ones will both be seen as yellow or blue and may be confused.

(3) If two colors are in Stage 3 so that the bright, saturated colors are seen as yellow, and the others as gray, confusions will be found to exist between all the reds and greens.

(4) If both are in Stage 4 confusion will be complete.

(5) If, on the other hand, red is in Stage 1 and is thus seen normally, and green is in Stage 2 and seen as green and yellow (or vice versa), no confusion will take place.

(6) If red is in Stage 1 and green in Stages 3 or 4 (or vice versa), being seen as yellow and gray, or pure gray, there will be no confusion because the color seen normally can always be distinguished.

(7) If one of the hues is in Stage 2 and the other in Stage 3, there will be some confusion between the shades and tints which are yellow, but not very much.

(8) If one is in Stage 2 and the other in Stage 4, there will be no confusion, since one is always colored and the other appears gray.

(9) If one is in Stage 3 and the other in Stage 4, confusion will take place between the color which appears gray in Stage 3 and that which appears gray in Stage 4.

(10) In cases where the green is not changed to yellow, but simply becomes a less saturated green, while the red is seen as yellow, there will be no confusions except when one is in Stage 3 and the other in Stage 4.

(11) Colors from opposite halves of the circle will not be confused unless both are in Stage 3 or 4.

These rules work out consistently, not only for the reds and the greens, but also for the hues which occupy the corresponding positions on the other half of the ring, namely, purple and blue-green.

(c) *The color ring.* From the vertical columns in Table II the average rating given the *S*s for each hue was computed. When these figures are placed in the color ring, several interesting points are evident (see Fig. 1). There are four points of importance on the ring; these have been labeled A, B, C, D. The ratings indicate that there are two points, A and C (the blue-

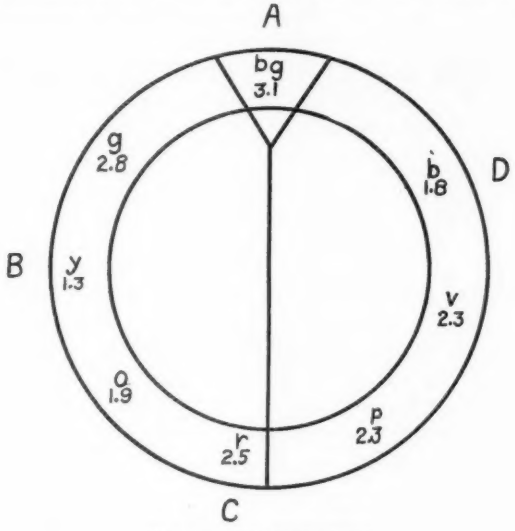


FIG. 1 THE COLOR RING

green and red), at which difficulty with color vision starts. From these two points difficulty spreads in both directions, but as we progress from points C and A to points B and D, color vision becomes progressively better. At B and D it is best. The whole thing is not symmetrical, however, for B is a point of much clearer vision than D. Blue-green is likewise (point A) more often confused than red. The numbers outside the smaller circle indicate the average grade of color vision for each hue: blue-green being most often confused, then green, red, purple, violet, orange and yellow in the order named.

These numbers, however, are derived from averages. Do all individual cases consistently follow this order? A study of Table II shows that they do, for almost every case is weakest at A,

gets better towards B, worse towards C, better towards D and worse towards A again. There are scarcely a dozen cases where this order does not hold true, that is, where a hue near to points A and C has a higher rating than one further out toward points B and D. These exceptions may be due to inaccuracies in the ratings.

TABLE III

TOTAL NUMBER OF CONFUSION ERRORS MADE BY ALL SUBJECTS INCLUDING THE NORMAL ONES

The first number in the squares indicates the number of times the two colors at the heads of the intersecting columns were confused, the second number is the average of confusions per card.

colors	r	o	y	g	gr	bg	b	v	p	x
br	*580 27.6	*50 3.6	34 2.6	160 7.3	66 4.4	10 0.5	21 1.4	14 1.0	*48 2.5	71 10.7
r		*202 9.6	18 0.9	153 5.3	153 6.9	44 1.8	32 1.5	72 3.4	*388 14.9	274 19.5
o			*111 8.5	64 2.9	13 0.8	0 0.0	6 0.4	11 0.8	14 0.7	122 17.4
y				*282 13.4	14 1.0	28 1.6	4 0.3	4 0.3	1 0.1	50 8.3
g					316 13.7	358 13.8	42 1.8	27 1.2	27 1.0	182 12.1
gray						386 20.3	171 10.7	271 18.0	117 5.8	91 11.3
bg							*526 27.6	241 13.4	167 7.3	94 8.5
b								*299 19.9	192 9.6	64 8.0
v									*588 29.4	80 1.1
p										165 13.7

*indicates a confusion of colors which are adjacent on the color ring, and therefore an error of the type made by the normal *Ss* as well as by the color-blind.

x is the number of times a color was left unclassified.

The circle is divided as shown. Blue-green belongs to neither side; purple may be seen as either red or blue. While there is a tendency for complementary colors to fade out in pairs there is no general rule. Sometimes, though rarely, one of a complementary pair is rated 4 and the other 1. This fact should have an important bearing on theories of color vision if substantiated, for they are all based on the grouping of colors into complementary pairs.

In general, if a hue is in Stage 4, the hues on each side will be found to be affected. In very mild cases of color-weakness, green seems to be the first color affected, though we have not enough cases to prove that this is always so.

(*d*) *Confusions compared.* *Ss* may be classified by their results in the color-sorting test into three groups, the normal, the color-weak, and the color-blind. Certain types of mistakes are typical of each group.

(1) The controls confuse colors which lie adjacent to each other on the color ring, and, occasionally, very light, dark, or unsaturated colors from the cold side of the ring are confused with gray.

TABLE IV

CORRELATIONS

Color-Sorting Test and Stilling's Tables.....	$+0.78 \pm .02$
Color-Sorting Test and Ishihara Test.....	$+0.82 \pm .02$
Stilling's Tables and Ishihara Test.....	$+0.81 \pm .02$

(2) The color-weak make the same sort of mistakes as the controls, but make many more of them. In addition there are occasional confusions of the sort the color-blind make. A loss of ability to distinguish some of the more confusing shades, and to follow a hue in the changes it goes through in the different values and saturations, is indicated.

(3) The color-blind make all the mistakes made by the other two classes and many more besides. Table III indicates confusions made by all *Ss*. It will be evident that every hue was confused with every other with the exception of blue-green and orange. Only 11% of confusions were between hues on opposite sides of the ring.

(*e*) *Correlations between tests.* Correlations were found between the Stilling and Ishihara tests and the color-sorting test.

In grading the results from Stilling's Tables and the Ishihara Test the method was simply to use the number of mistakes as the score. For the color-sorting test the score was the *S*'s average from Table II—that is, the average rating he received according to the Four-Stage classification. Table IV gives the correlations.

Correlations of +0.80 between this test and the others suggest rather more agreement than really exists in some instances. The

tests disagree very seriously in selecting the 'totally green blind' and the 'totally red blind.'

The Ishihara test uses a purple number on a gray background to select the green-blind. Evidently the theory is that the totally green-blind always have a neutral band in the purple. According to the color-sorting test, this is not true. There are 9 *Ss* who are rated a 4 in the green. Of these only 4 show evidence of seeing purple as gray also. The Ishihara test was successful in picking out all of these four. Two *Ss* have a gray band in the green according to the sorting test, but are not classed as green-blind on the other test. What is more serious, 14 out of 22 *Ss* classed as green-blind on the Ishihara test showed no evidence of being completely blind to the green. *S* 10 is even rated as 1 in the green.

If we assume that these cards with their purple number on the gray background fail to select the green-blind, but pick out those with a neutral band in the purple, we are no better off. Of 22 *Ss* who failed to read the numbers, only 6 clearly saw gray instead of purple. If we include those whose purple is in Stage 3, who see some purple as blue, the number is raised to 11.

Numerous other cases could be cited. How can we explain the fact that an *S* can identify, name, and classify correctly nearly all the red cards in a pack, and yet cannot read a bright red number on a gray background which is as clear as day to the normal eye? And how explain the fact that some *Ss* failing to read the number could actually name the colors of the dots?

W. R. Miles suggests that the area on the retina capable of receiving the sensations of red and green may in such cases be very much shrunken from the normal. In that case an *S* could see a spot of color, or could identify the hue of a large area of color, but would be unable to see all of a 2-in. number at once, for most of it would appear as uncolored. This hypothesis is substantiated by the fact that some *Ss* were found who could not read the figures except at a distance of several feet. Also one of the *Ss* who could sort colors but could not read the figures was tested with a perimeter; her field for the red and green was found to be shrunken to a tiny area around the fovea. Another possibility would be that the part of the retina sensitive to red and green had been broken up into irregular sections, some color-sensitive and some color-blind, so that the whole number would not be registered.

Another explanation is that the number cards use, for the most part, somewhat pale, unsaturated colors, rather than standard spectral ones. We have shown that changes in value and saturation are highly important. Because an *S* fails to read a pink letter on a pale green background, we cannot therefore conclude that he will always confuse red and green. Similarly, because he does not see a grayed purple number on a gray background, we cannot leap to the conclusion that he has a neutral band in that hue.

Whether the explanation here offered is correct or not, it would certainly not be right to discard the number tests on the ground that they do not give the same results as the color-sorting tests. The difference is due to the fact that two quite distinct situations are presented to *S*, in one case a field of unbroken color, in the other small dots of different colors. The latter situation is far more often met with in real life than the former. In looking at a picture

or a landscape, color is seen in small broken areas, rather than in large unbroken ones. The numbers cannot be neglected in testing color vision, for they offer a unique contribution.

IV. Future Work

A more objective method of testing the truth of the Four-Stage classification is desired. A possible technique would be to use number cards with a wider variety of colors. Four or five pairs of complementary hues would be necessary. Six of each hue, in three values, with two saturations of each value, would be necessary. The warm hues would then be used to paint numbers on backgrounds of appropriate grays and yellows,the cold numbers on backgrounds of blues and grays. Blue and yellow combinations would also be necessary. A hundred such cards would form a thorough, rapid, and objective means of testing out the Four-Stage classification described above.

V. Summary

(1) The color sphere rather than the spectrum should be used as a basis for tests of color-blindness, since differences in saturations and brightness are for the color-blind differences in hue.

(2) An individual's color vision may be described in terms of the Four-Stage classification of color vision. Each hue is given a rating of 1, 2, 3, or 4. A rating of 1 indicates perfect vision in that hue, a 4, that the hue is seen as gray. A 2 means that the bright saturated colors of the hue are seen in their true hue, though somewhat less saturated than they appear to the normal eye, while the light, darker, and less saturated colors are seen as the central unit color of the side of the spectrum on which they lie; that is, as yellow or blue. In Stage 3 the saturated colors are seen as either yellow or blue and the others as gray. Occasionally a color does not change in hue, but is seen merely as a weaker saturation of the true hue.

(3) The many different types of confusions made by different color-blind individuals are explained by this classification, for only colors which are seen alike will be confused.

(4) On the color ring there are four critical points. Color vision is poorest at blue-green, grows progressively better as we proceed to yellow, grows worse again to red, better as far as blue, and worse again till blue-green is reached.

THE INFLUENCE OF DEGREE OF LEARNING UPON RETROACTIVE INHIBITION[1]

By JOHN A. McGEOCH, University of Arkansas

Several studies have dealt with the relation between the degree to which a given material has been learned and its susceptibility to disintegration through retroactive inhibition. In seven different experiments, each of which compared two degrees of learning, Heine[2] found greater inhibition with fewer repetitions of the syllables to be learned. Robinson,[3] using 4, 6, 8, 10, and 12 repetitions of three-place numbers, obtained irregular results, indicating no single continuous relation between number of presentations and degree of retroaction. The findings of Robinson and Heron[4] and of Robinson and Darrow[5] that, with nonsense syllables and lists of numbers, susceptibility to retroactive inhibition decreases as the lists increase in length, suggest an inverse relation between degree of learning and retroaction, since the longer lists were also the better learned. Whether there exists any continuous relation between degree of learning and amount of retroaction is not, however, known.

PROBLEM AND METHOD

The problem of the present study is to investigate the influence of degree of learning upon amount of retroactive inhibition, when widely varying numbers of repetitions of the memory material are used.

Lists of 9 nonsense syllables each were exposed on a Chicago memory drum at the rate of 2 sec. each, and were learned by the anticipation method, the *Ss* being asked to anticipate the succeeding syllables by spelling instead of by

*Accepted for publication August 1, 1928.

[1] Research Paper No. 87, Journal Series, University of Arkansas.

[2] R. Heine, Über Wiedererkennen und rückwirkende Hemmung, *Zsch. f. Psychol.*, 68, 1914, 161-236.

[3] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, no. 128, 1-57.

[4] E. S. Robinson and W. T. Heron, Results of variations in length of memorized materials, *J. Exper. Psychol.*, 5, 1922, 428-448.

[5] E. S. Robinson and C. W. Darrow, Effect of length of lists upon memory for numbers, this JOURNAL, 35, 1924, 235-243.

pronouncing them. A seconds-pendulum, hung on the wall behind the *S* but in full view of *E*, timed the exposure rate. *E* exposed the next syllabl e : time the pendulum appeared at a given point in his visual field.

Five different numbers of presentations were used: 6, 11, 16, 21, and 26.[6] At the beginning of the experiment the *S*s were given a practice series of three days, on each of which they learned two lists. The experiment proper consisted of two cycles, in each of which the *S*s went through each number of repetitions once. The *S*s were put through the different degrees of learning, at the rate of one per day, in a counterbalanced practice order such that all numbers of presentations had equal average position. On each day *S* went through both a rest and a work condition, the same number of repetitions being used in both conditions. Five *S*s took the rest condition first each day in Cycle I and the work condition first each day in Cycle II, and five reversed this order.

Under the rest condition *S* was given the desired number of repetitions, followed by a 5-min. period of no learning activity, during which he was asked to read silently from a humorous magazine, marking on each page the three jokes which he considered best. Since most of the *S*s were relatively inexperienced and would have found it impossible to maintain a perfect rest period of no rehearsal unaided, this method was used to insure as much as possible against review of the syllables during the rest interval. Eight of the *S*s considered the marking of the jokes a second part of the experiment. Their behavior while reading, and their verbal reports at the conclusion of the experiment, indicate that the lists were seldom recalled, even fragmentarily, during the rest intervals. At the end of the 5-min. period the list was relearned to one perfect repetition.[7] Under the work condition, *S* was given the desired number of presentations of a list, followed by a 30-sec. interval during which a second list was prepared for exposure. Immediately following this *S* was given 11 repetitions of a second list of 9 syllables. The interpolation of 11 repetitions of a second list was constant for all of the work conditions of the experiment. The remainder of the 5-min. interval, not occupied by the learning of the interpolated list, was filled by reading, as in the rest condition, from a humorous magazine. At the end of the 5-min. interval the original list was relearned to one perfect repetition, and one presentation of the interpolated list was given in order that *S* might not gain the impression that the learning of this list was less important than the learning of the first list.

All *S*s went through both cycles in the same room, with the memory drum in the same position, with the same *E* and as nearly as possible at the same time of day.

Ten *S*s, 6 men and 4 women, went through both cycles. Eight were students in experimental psychology, with some previous practice in this type of experimentation. Two were members of the department of psychology.

[6]It was planned to use one repetition as a sixth degree of learning, but preliminary work gave results too irregular to be of significance, and the one-repetition condition was omitted.

[7]The term 'relearning' is a misnomer in the case of the smaller numbers of repetitions, since few of the *S*s mastered the material to the extent of one perfect series of anticipations. In such cases, the original learning simply continued after the interval.

Treatment of Data. Since equal numbers of learning trials have been given for rest and work and since we are comparing throughout records on the same group of *S*s, it is possible to compare directly the retention scores under the two conditions. The methods of scoring the retention and of finding the amounts of retroactive inhibition are as follows.

(1) The total number of syllables correctly anticipated at the first relearning presentation has been found for each degree of learning under each condition. These scores will be referred to as recall scores. The difference between the recall scores for the rest and work conditions represents the absolute amount of retroactive inhibition at each degree of learning.

(2) Inasmuch as it is more important, however, to consider in our measure of retroaction the amounts of the scores between which differences obtain, the relative amount of retroaction is the significant score. This is represented by the percent which the difference between the rest and work conditions is of the score under the rest condition. The rest condition is obviously the standard against which the work results must be judged.

(3) The total number of trials required for relearning to one perfect repetition has been found for each condition and each degree of learning. The difference between the rest and work scores gives the absolute amount of retroaction, at each degree of learning, in terms of relearning trials required.

(4) Saving scores have been computed from the data of (3) by the formula:

$$\text{Saving Score} = 100 - \left(\frac{\text{No. Presentations Required to Relearn} - 1}{\text{No. Learning Presentations}} \times 100.\right)$$

The -1 in the numerator is to deduct the last and perfect repetition. It is absent in the denominator, because the number of presentations on any experimental day is fixed and, with the smaller numbers of repetitions, is not always sufficient for complete mastery. The absolute amount of retroaction is, of course, the difference between the rest and work saving scores.

(5) Relative retroaction in terms of saving has been found by subtracting the work from the rest scores and dividing by the rest score.

(6) In order to find the degree of learning, as an auxiliary measure to the number of presentations given, an overlearning ratio has been computed for each list by the formula: $\text{Overlearning Ratio} = \frac{\text{Anticipations}}{\text{Failures to Anticipate}} \times 100.$

Results

The total recall scores appear in Table I. Each figure in this and in all later tables has been obtained from 20 individual records.

TABLE I

Syllables Correctly Anticipated at First Relearning Presentation

	Number of Repetitions in Learning				
	6	11	16	21	26
Rest	73	102	108	143	134
Work	13	22	38	55	72
Difference	60	80	70	88	62
D/R	82.2%	78.4%	64.8%	61.5%	46.2%

The total recall scores increase with increasing numbers of learning presentations, save from 21 to 26 repetitions under the rest condition. The increase is more regular under the work than under the rest condition. The differences between the scores under

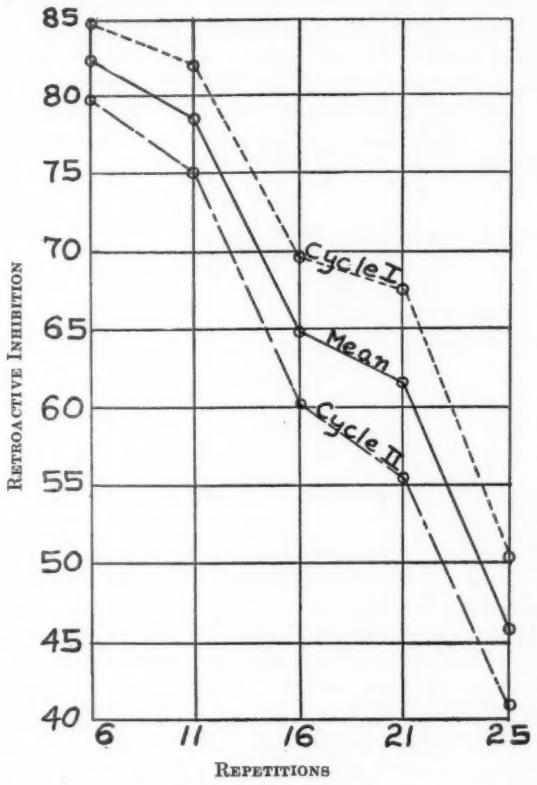


Fig. 1. Relative Retroactive Inhibition Recall Score

the two conditions show an irregular relation between number of presentations and gross retroaction. When, however, the relative amount of inhibition is considered, the relation becomes much more regular. The relative inhibition for each cycle of the experiment and for the two cycles combined is shown in Fig. 1.

The relative amount of inhibition is high, when only 6 repetitions are given the material to be learned, and decreases steadily as the number of presentations increases. Even when 26 repetitions are given, however, 11 repetitions of an interpolated list are sufficient to effect a 46.2% disintegration. The slope of the curve is less between 6 and 11 and between 16 and 21 repetitions. This may be the result of chance factors, or it may indicate that for retroactive inhibition there is a smaller difference between these degrees of learning than between 11 and 16 and between 21 and 26 repetitions. The agreement between the curves for the two cycles argues for the latter possibility.

TABLE II

TRIALS TO RELEARN

Number of Learning Repetitions

	6	11	16	21	26
Rest	74	50	35	10	11
Work	124	90	72	53	38
Difference	50	40	37	43	27

TABLE III

SAVING SCORES

Number of Learning Repetitions

	6	11	16	21	26
Rest	38.4	77.3	89.1	97.7	97.9
Work	−03.3	59.1	77.5	87.4	92.7
Difference	41.7	18.2	11.6	10.3	05.2
D/R	108.5%	23.5%	13.0%	10.5%	05.3%

The curves for the two cycles agree closely in form. Retroactive inhibition is greater, at every number of repetitions, during the first cycle than during the second, indicating that practice tends to decrease the susceptibility to retroactive inhibition.

Both from the curves for the individual cycles and from the composite curve it is evident that, when retention is measured by the number of syllables correctly recalled at the first relearning presentation, the relative amount of retroactive inhibition varies inversely as the number of presentations given the material to be learned.

The total numbers of trials required to relearn to one perfect repetition, the mean saving scores computed from them, and the amounts of retroactive inhibition are given in Tables II and III.

It is clear that the total number of trials required for relearning decreases, under both rest and work, as the number of learning

trials increases. The fact that the rest relearning trials at 21 and 26 learning presentations are practically equal is a slight exception to this statement. Fewer relearning trials are required under rest than under work, and the difference between the two conditions decreases from 6 to 16 repetitions, rises at 21 repetitions,

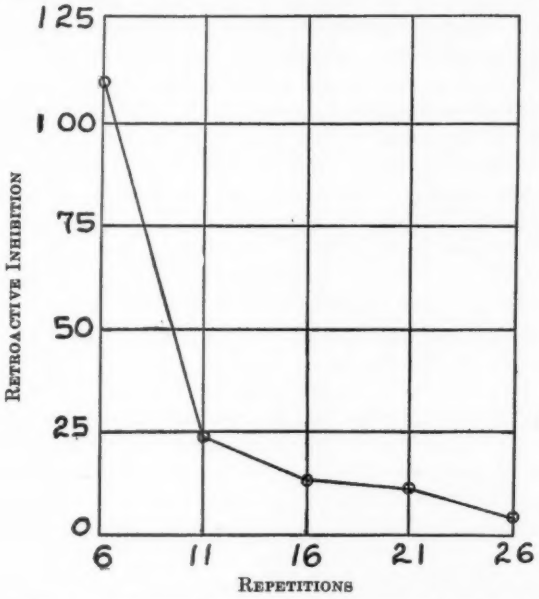


FIG. 2. RELATIVE RETROACTIVE INHIBITION SAVING SCORE

and is lowest at 26. In terms of the absolute difference between the relearning trials under the two conditions, there is, then, a definite though somewhat irregular tendency for retroactive inhibition to decrease as the number of learning presentations increases.

The mean saving scores, of course, increase steadily with increasing numbers of repetitions, under both rest and work conditions, and the difference between the scores under the two conditions decreases consistently. The percent which this difference is of the mean rest scores, or the relative amount of retroactive inhibition, is shown graphically in Fig. 2.

When only 6 repetitions are given the material to be learned, and when 11 repetitions of a second list are interpolated between the learning and recall of the first list, not only is nothing saved, but a slight loss occurs. The relative inhibition is, therefore, more than 100%. The difference between 6 and 11 repetitions, in terms of relative retroaction, is 85%, giving a very sharp drop in the curve. From 11 repetitions the curve falls slowly and regularly to a relative inhibition of about 5% at 26 repetitions.

The curves for the individual cycles are so close together that they are difficult to distinguish, when drawn. Only the composite curve is given, therefore, in Fig. 2. Cycle II shows the greater inhibition at the first three points and the less inhibition at the remaining two points. Thus, no consistent relation appears between practice and degree of retroaction.

The conclusion is clear that, measured in terms of saving score, retroactive inhibition varies inversely as the number of presentations given the material to be learned. The curve of inhibition plotted against number of learning repetitions shows marked negative acceleration.

Corroborative evidence of the validity of the conclusions drawn is given by the fact that the two *Ss* who were trained both in the use of the anticipation method and in general psychological experimentation, and who reported practically perfect rest periods throughout the experiment, give curves for both recall and saving retroaction which are closely similar to those of the eight untrained subjects.

It is possible that increase in the number of repetitions given the nonsense syllable lists might not have meant a corresponding increase in the degree of learning. Consequently, the overlearning ratios for each *S* have been computed. The mean ratios for the rest and work conditions, and for the interpolated learning, are shown in Fig. 3. The rest and work overlearning ratios increase as the number of repetitions, with a slight negative acceleration. Clearly, increase in the number of presentations represented a corresponding increase in the degree of learning—or of overlearning. There is no apparent reason for the small but uniform difference between the ratios under the two conditions. The lower ratios of the work condition may have served to accentuate the degree of retroaction, but it can hardly have affected the tendency of the results.

The rise in the curve for interpolated learning from 6 to 21 repetitions, small though it was, was hardly to have been expected. One would expect fluctuations, but not regular increases. It is un-

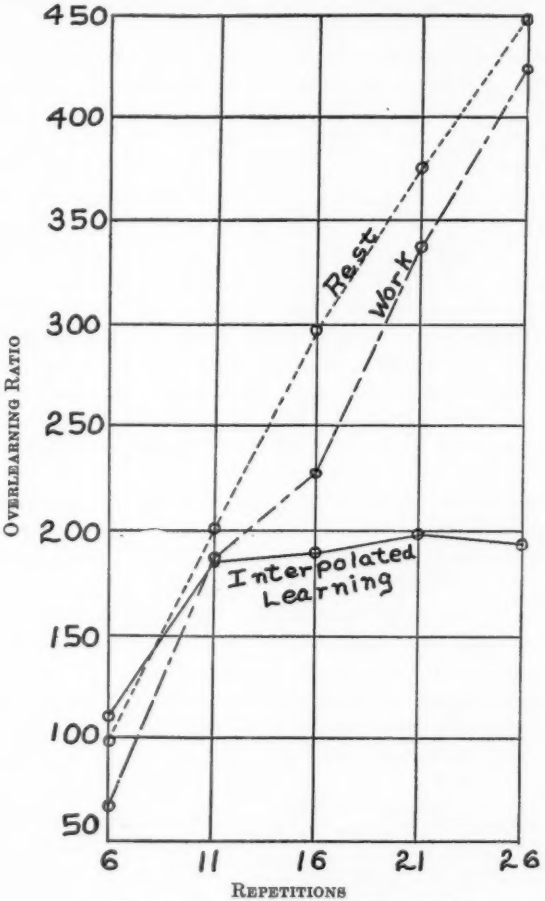


Fig. 3. Mean Overlearning Ratios

likely that it is the result of practice, since all positions on the curve had equal average position in the experiment. The regularity is not likely to be the result of chance factors. It may be the

outcome of a 'set' established by the number of repetitions given the original list in the work condition. When more repetitions were given the first list, the *S*s may have become 'set' for harder work on the interpolated list, with the result that it was better learned.

The amounts of retroactive inhibition found in this experiment, especially when retention is measured in terms of recall scores, are higher than the amounts usually reported. This difference needs, perhaps, some explanation. The first possibility is, of course, rehearsal of the syllables during the rest interval. This may, however, be discarded with some confidence for the following reasons: (1) during the rest interval, the *S*s were obviously absorbed in the reading of the jokes; (2) the *S*s uniformly reported no rehearsal; and (3) the relative retroaction curves for the 2 practiced *S*s, who reported freedom from even fragmentary recall during rest, are closely similar to those of the 8 unpracticed *S*s. That the differences between the overlearning ratios under rest and work may have accentuated the degree of retroaction somewhat has already been noted. It is, however, extremely unlikely that this difference could have led to any great increase in retroaction.

The amounts of inhibition with the smaller number of learning presentations, when retention is measured in terms of recall scores, are the only ones needing consideration in this connection. Retroaction with saving scores is high only with 6 learning repetitions. A comparative analysis of this and other studies in retroactive inhibition reveals differences in procedure which probably account for the differences in retroaction.[8] Such analysis is hardly worth giving in detail, but the principal factors contributing, presumably, to differences in retroaction may be cited. (1) In several studies nonsense syllables have not been used, or

[8] J. E. DeCamp, A study of retroactive inhibition, *Psychol. Monog.*, 19, 1915, no. 84, 1-69; W. Hinrich, Zur Frage der rückwirkenden Hemmung, *Zsch. f. Psychol.*, 99, 1926, 389-392; G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. 1, 1900, 1-300; E. S. Robinson, The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312; E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, no. 161, 1-69; E. C. Tolman, Retroactive inhibition as affected by conditions of learning, *Psychol. Monog.*, 25, 1917-18, no. 107, 1-50; P. L. Whitely, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psychol.*, 10, 1927, 489-508; also Heine, Robinson et al, *opp. citt.*

have been used in but one or two variations of the experiment. (2) It is not always possible to state the exact degree of learning used in some experiments, either because of the nature of the material or because of the learning method used. It is, however, doubtful if degrees of learning as slight as those represented in the 6 presentations of this experiment have often obtained in other experiments. (3) The 2-sec. exposure time used here differs from that sometimes used. (4) The anticipation method of this experiment differs from the learning methods often employed. (5) The type of interpolation used was designed to favor a high degree of retroaction. Presumably, interpolated nonsense syllable lists have a greater disintegrative influence upon other nonsense syllable lists than do lists of numbers or adding, for example. (6) Further, the interpolated list followed the original learning almost immediately. This probably favors retroaction. (7) The methods of measuring retention differ from those of several other experiments. It is probable that these differences, several of which obtain between this and each other experiment upon retroaction, explain very readily the different amounts of inhibition found. The experimental conditions were arranged to produce a high degree of retroaction, and then to measure the influence of degree of learning upon it. The amounts discovered seem to be, then, a function of the particular experimental conditions.

The results of this experiment have no immediate bearing upon either the perseveration theory or the transfer theory of retroaction. They may be incorporated into either theory with equal ease.

Summary

When 9-syllable lists are learned by the anticipation method, with a 2-sec. exposure, to 6, 11, 16, 21, and 26 repetitions, and when at each number of presentations a rest condition is compared with a work condition in which an interpolated 9-syllable list is learned similarly to 11 repetitions, the following results appear.

(1) Measured by the overlearning ratio, the increase in number of presentations is accompanied by a corresponding increase in the degree of learning until, with the larger numbers of repetitions, overlearning is very great.

(2) Increase in the number of repetitions to as many as 26 yields an increase in retention, measured by both recall, relearning

and saving methods. This is true of both rest and work conditions, with but one slight exception.

(3) The relative amount of retroactive inhibition, when retention is measured by the recall method, varies inversely as the number of presentations given the material to be learned. It is particularly significant that a list learned even to 26 repetitions, with correspondingly high overlearning ratio, is unable to resist the disintegrating influence of retroactive inhibition from a list of equal length learned to 11 repetitions. With the 26 repetitions there is still a disintegration of 46.2%. Practice tends somewhat to reduce the susceptibility to retroaction as shown by recall. The absolute amount of retroaction, measured by the difference between rest and work scores, shows no constant relation to degree of learning.

(4) The relative amount of retroaction, when retention is measured by the saving method, varies inversely as the number of learning presentations. The curve of inhibition plotted against the number of repetitions for learning shows marked negative acceleration, and falls from 108.5% at 6 presentations to 5.3% at 26 presentations. The absolute differences between rest and work scores for relearning and saving tend also to decrease with increasing learning.

RELIABILITY OF THE GALVANIC REFLEX*

By Alvhh R. Lauer, Ohio State University

Since the first use of the galvanic reflex for measuring emotional reactions by Féré[1] in 1888, many investigators have studied the phenomena. Various measurements of bodily changes, memory, effects of alcohol, atropin, and other drugs on the organism, anaesthesia, and sleep have been made with the galvanometer. Further, it has been used as a complex indicator and in clinical diagnosis of mental disorders. Other uses of the galvanic reflex have been suggested and investigated to some extent; notably in the experimental studies of Marston on emotional reactions, Darrow[2] on secretory changes in the skin, and Wechsler,[3] who studied general correlated effects. Without commenting on the merits of these experiments or the nature of the reflex, it suffices to say that only in a general way has any standardized technique been employed, and usually results are not comparable. For the purpose of convenience we shall adopt conventional terminology and speak of changes in potential as changes in body resistance, although this practice has been shown to be seriously at fault by Gildemeister[4] and discussed at some length by Prideaux.[5]

Problem. While working on the major problem of investigating bodily changes concomitant with the galvanic reflex, the writer made a careful search of the literature for a satisfactory technique. Different methods have been used with some degree of success but little has been said concerning the reliability of results obtained.

*Accepted for publication September 6, 1928. The experimental work was done at Iowa State College.

[1]C. H. Féré, Note sur les modifications de la résistance électrique sous l'influence des excitations sensorielles et des émotions, *C. r. Soc. biol.*, 43, 1888, 217.

[2]C. W. Darrow, Sensory, secretory, and electrical changes in the skin following bodily excitation, *J. Exper. Psychol.*, 10, 1927, 197-226.

[3]D. Wechsler, The measurement of emotional reactions, *Arch. Psychol.*, 12, 1925, no. 76, 5-181.

[4]M. Gildemeister, Der sogenannte psycho-galvanische Reflex und seine physikalisch-chemische Deutung, *Arch. f. d. ges. Physiol.*, 162, 1915, 489-506.

[5]E. Prideaux, The psychogalvanic reflex: a review, *Brain*, 43, 1920, 50-73.

Before the validity of a test can be established the reliability of such measurement must be determined. If the test will not measure twice the same way, that is, if the reliability is low, then its use is not warranted. Very few investigators have even mentioned the reliability of the galvanic reflex. Smith[6] has reported correlations of first with second, third, fourth, fifth, or sixth tests of the reflex, ranging from +0.69 to +0.01, giving a mean r of +0.36. Correlations between means of groups gave considerably higher indices. The mean correlation of subjects on the same stimuli was +0.08. However, Smith used words only as stimuli and one would expect that verbal material would have greater specific meaning, thus giving a more consistent affective tone value, than more general or varied stimuli. Syz[7] obtained a significant correlation with subjective self-ratings, but he was dealing rather with the validity of the deflections. It is to be expected that the reliability would be at least as high as the validity. Wechsler[8] found that different types of stimuli inter-correlate rather highly, the average or mean inter-correlation being +0.50.

Smith[9] has outlined 7 factors which may influence the absolute magnitude of galvanometer deflections from a given emotional or sensory stimulus. (1) Intensity of the emotion actually evoked. (2) The proportion of it which finds expression through those efferent channels which innervate the skin mechanism responsible for the reflex. (3) The responsiveness of the skin to such innervation. (4) The initial resistance of the skin. (5) The sensitivity of the galvanometer. (6) The magnitude of the fixed resistance of the bridge (using Féré's method). (7) The E. M. F. applied to the bridge.

The writer wishes to add another factor, namely (8) the kind of electrode used and the nature, extent, and constancy of the contact with the skin.

The purpose of the present study was (a) to hold as nearly constant as possible all the above factors except (1), *i.e.* the absolute magnitude of galvanometer deflections; (b) to attempt to equate these deflections in terms of initial resistances and cer-

[6] W. W. Smith, *The Measurement of Emotion*, 1922, 30-40, 113-120.
[7] H. Syz, Psychogalvanic studies on sixty-four medical students, *Brit. J. Psychol.*, 17, 1926, 54-69.
[8] *Op. cit.*
[9] *Op. cit.*

tain physical relationships of the apparatus; and (c) to establish the maximum possible reliability from the equation of (a) and (b).

Apparatus. One of the first essentials in any objective measurement is a reliable instrument, as no measurement can be more exact than the accuracy of the instrument. One of the first points of attack was the eighth factor given above, the kind of electrode used. Through suggestions offered by physicists a new type of electrode was devised which has been endorsed by electricians and experimental psychologists. This electrode has been described elsewhere.[10]

The apparatus was patterned after that of Smith[11] and modified by valuable personal suggestions from McCarthy.[12] A d'Arsonval galvanometer of 500 ohms resistance and 300 megohms sensitivity with a period of 3 sec. was used in connection with a Wheatstone bridge. The fixed resistances of the two arms of the bridge were 100 and 1000 ohms respectively, making the ratio of the known variable resistance to the X resistance 1 to 10. An exosomatic current of 3.2 volts was passed through the bridge. A shunt of 3 ohms was placed across the terminals of the galvanometer and this was kept constant throughout the experiment. *S* was placed in an enclosed booth and the electrodes attached. After a few seconds the bridge was balanced and the stimulus given. The deflections were recorded after each stimulus.

Stimuli. The stimuli employed were of varied nature and as nearly as possible were administered in a mechanical way to eliminate the influence of a third person. In the case of all excepting the olfactory stimuli this precaution was taken. In the latter case *S* was allowed to inhale an odor from a vial held by the same person in exactly the same way. The curtain was withdrawn for each stimulation after *S* had been told the nature of the stimulus. With all other stimuli he was not warned. Thirty-three different types of stimuli were used, ranging from questions from the Woodworth Psychoneurotic Questionary to mental arithmetic problems and varied sensory stimulation.

The conditions were controlled as nearly as possible and the same stimulus was given to the same person in the same way at two different times. The results of the two tests were treated statistically and a number of relationships worked out as listed in the tables below.

Results. There were four possible factors to consider in evaluating a given deflection of the galvanometer: (1) the initial bodily resistance; (2) the magnitude of the deflection; (3) the ohmic change in initial resistance; and (4) the percentage or ratio of change in resistance to the initial resistance. The question naturally arose as to which was the most practical to use as a measure of emotional response. This, it is assumed, would be the measure which would give the highest degree of reliability, show least variability, and be more nearly independent of other variables. Since the other factors mentioned by Smith

[10]A. R. Lauer, A new type of electrode for the galvanic skin reflex, *J. Exper. Psychol.*, 11, 1928, 248-152.

[11]*Op. cit.*

[12]R. McCarthy, *The Measurement of Conation*, 1926, 29-47.

were held relatively constant, the next step was to calibrate the instrument and determine the significance of a given deflection with a given initial resistance. To do this a complete set of resistances was procured, and an electrical engineer aided in calibrating the apparatus. Various known resistances were substituted for S, and the apparatus was calibrated from these. It was

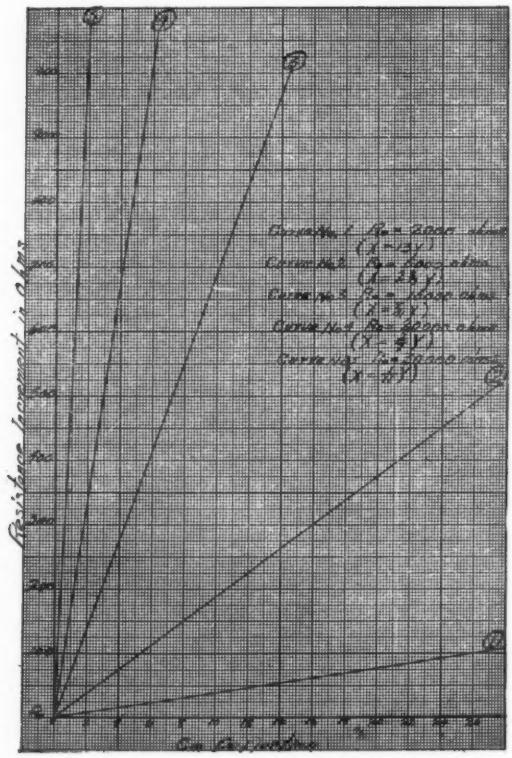


FIG. 1. DEFLECTIONS OF GALVANOMETER WITH THE CHANGE IN RESISTANCE

found that when the resistance in the bridge was held constant the deflections of the galvanometer gave a linear relationship with the change in resistance (see Fig. 1). This was merely a test of the accuracy of the galvanometer, since it was constructed to give this relationship. When the resistance in the bridge was changed, however, this function was altered, as may be noted from Fig. 2.

The data were now studied and self-correlations between significant aspects of the measure were calculated. According to Garrett[13] the index of reliability is the highest possible correlation which could be expected between obtained scores on a given test and the true scores on the test. In all cases where self-correlation was used for establishing the reliability of a measure

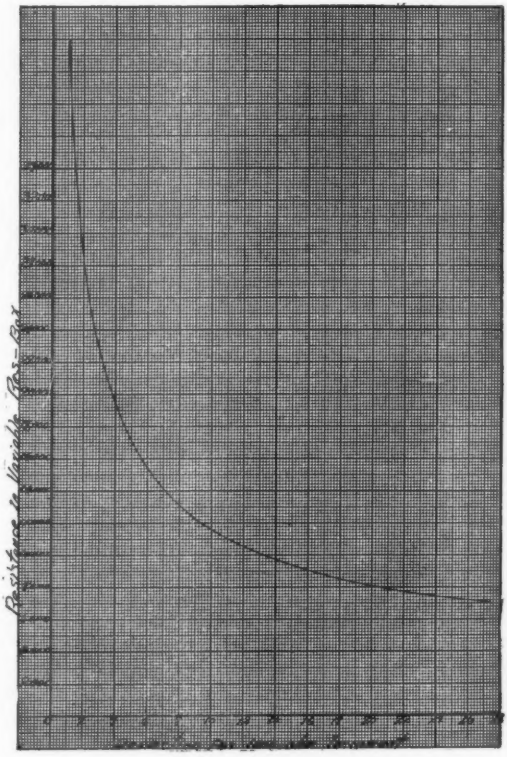


FIG. 2. DEFLECTIONS OF GALVANOMETER WITH CHANGES IN THE RESISTANCE OF THE BRIDGE

these values also have been calculated. The means, sigmas, and coefficients of variability have also been computed to aid in properly evaluating the data.

The order followed in the treatment of results has been as follows: (1) to determine gross reliability of the total 33 stimuli (Table I) obtained from 22

[13]H. E. Garrett, *Statistics in Psychology and Education*, 1926, 273.

Ss on 348 reactions; (2) to find the reliability of 4 specific stimuli used in a study of the validity of the galvanic reflex taken alone and then combined (Table II); (3) to study the reliability of the reflex on one *S*, using several stimuli in repeated reactions (Table III); and (4) to find the correlations of various related factors involved (Table IV).

TABLE I

RELIABILITY COEFFICIENTS FROM 348 REACTIONS OF 23 *Ss* USING 55 DIFFERENT STIMULI

r = raw coefficient of correlation on duplicate test; R = index of reliability; M_1 = mean of first measure; M_2 = mean of second measure; $S.D._1$ = standard deviation of first measure; $S.D._2$ = standard deviation of second measure; C_1 = coefficient of variability of first measure; C_2 = coefficient of variability of second measure.

Measure	r*	R	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2
Deflections	0.6188	0.78	8.16	5.66	0.69	7.72	6.15	0.79
Ohmic change	0.5223	0.72	1501.00	971.00	0.64	1342.00	1191.00	0.88
% change	0.5528	0.74	8.54	3.78	0.67	8.63	5.98	0.69
Mean	0.5653	0.748			0.67			0.79

*Correlations have been carried to four places to insure accuracy in calculating multiple Rs by the method of Tolley and Ezekiel.

TABLE II

RELIABILITY OF GENERAL STIMULI USED WITH 3 *Ss*
(For explanation of captions see Table I)

S	Reactions	r	R	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2
O	37	0.6420	0.80	6.83	8.27	1.2	7.00	7.32	1.1
S	22	0.3602	0.60	10.23	7.76	0.75	6.46	7.61	1.1
F	31	0.6474	0.81	9.13	7.58	0.83	4.91	4.91	1.0
Mean		0.5449	0.73			0.93			1.06

TABLE III

CORRELATIONS OF FACTORS

Initial resistance and deflection	−0.0924	±0.03
Initial resistance and ohmic change	+0.3322	±0.03
Initial resistance and percentage change	+0.0071	±0.03
Self-correlation of initial resistance	+0.8843	±0.01
Percentage change and deflection	+0.8843	±0.01
Deflection and percentage change	+0.8990	±0.01

TABLE IV

COMPARISON OF FIRST AND SECOND REACTIONS
(For explanation of captions see Table I)

Measure	M_1	$S.D._1$	C_1	M_2	$S.D._2$	C_2	Diff. in Means	P.E. Diff.	Ratio
Initial ohms resistance	17691	13295	0.75	17883	12720	0.71	192.0	941.00	0.21
Deflections	8.16	7.72	0.94	5.66	6.15	1.10	2.5	0.49	5.10
Ohmic change	1501.00	1342.00	0.89	971.00	1191.00	1.20	530.0	0.92	5.76
% change	8.54	8.63	1.00	5.78	5.98	1.00	2.7	0.54	5.10

TABLE V

SUMMARY OF RESULTS ON SPECIFIC STIMULI
(For explanation of captions see Table I)

Stimuli	r	R	C_1	C_2	C_m
Bell	0.6904	0.83	0.84	0.92	0.88
Skull	0.7774	0.88	0.99	1.08	1.03
Whistle	0.8169	0.90	1.16	1.20	1.18
Rifle	0.5701	0.75	0.98	0.79	0.84
Mean	0.7137	0.84	0.97	0.99	0.98

TABLE VI

RELIABILITY OF MEASURES IN ORDER OF MERIT
(For explanation of captions see Table I)

Measure	r	R	C_1	C_2	Cm	Ratio of P.E. of Diff. to Diff. in M_1 and M_2
% change	0.7012	0.84	0.85	0.83	0.84	5.11
Deflections	0.6593	0.81	0.91	1.00	0.96	5.10
Ohmic change	0.7104	0.84	1.02	1.04	1.03	5.76
Initial resistance	0.8843	0.94	0.75	0.71	0.73	0.21

Conclusions. (1) The percentage change or ratio of apparent ohmic change to initial bodily resistance seems to be the best measure of the galvanic response to sensory stimuli for the following reasons. (a) It is more reliable than deflections, with which it correlates +0.899. (b) The initial resistance has no effect on this measure, since the correlation was +0.007. (c) It is less variable than either of the other measures and hence would have a higher predictive power, other things being equal. (d) The variability of the first and second tests are more nearly equal. (e) It is more nearly independent of other influences measured.

(2) The relation between deflections and initial resistance is −0.092, or slightly negative. Smith[14] found a negative correlation between these two variables of −0.497. The present study would indicate that this negative relationship is a function of the apparatus and the resistance of the subjects used. The high variability of absolute deflections makes them less valuable as a unit of measurement.

(3) Ohmic change shows rather higher reliability than do deflections, but it is not desirable as a measure for two reasons. (a) It correlates with initial body resistance +0.332, and a spurious self-correlation is obtained since initial resistance has a reliability of +0.94. (b) It is quite variable and hence not good as a predictive measure.

[14] *Op. cit.*

(4) Generally, the galvanic reflex shows great variability since the instrument is delicate.

(5) The initial resistance could not be considered as a measurement since it is taken before the stimulus is given. It would not be wise even to use it as a factor of any weight. The use of percentage change allows for influence due to large differences in initial resistance.

(6) When all measurements are included there is considerable difference in variability between first and second tests. The reactions are smaller on the second test, probably due to mental set. For valid results it seems that first reactions are best. Moderately intense stimuli are most reliable.

(7) The reliability of the galvanic reflex is quite high and apparently higher than that of pencil and paper emotional tests now on the market. The validity must be thoroughly established in order to make the galvanometer an asset to psychological or physiological method.

(8) A possible increase in reliability could perhaps be obtained by using a formula for ascertaining the best weights and then combining different aspects of the measure, but the absence of data on the validity of the galvanic reflex makes this impracticable at the present time.

UNCONDITIONED SALIVARY RESPONSES IN MAN

By A. L. Winsor and T. L. Bayne, Jr., Cornell University

In man the quantity of saliva secreted by the parotid glands may show great variation in any given period.[1] This variability is due to the numerous sources of excitation and inhibition, either direct or indirect, conditioned or unconditioned. Any study of the conditioned responses involves necessarily an exploration of the unconditioned responses, in order that the latter may be controlled or given consideration. In our preliminary research in this field we noted what appeared to be other sources of direct stimulation in addition to the mechanical, chemical, and protopathic factors accounted for by Lashley. When all other sources of excitation were accounted for, the muscles of mastication and swallowing seemed to cause measurable disturbances. The purpose of this article is to report some of the data from our studies which would seem to indicate proprioceptive stimulation of the parotid glands.

The obvious difficulty in a study of this sort is to single out one muscle or group of muscles for action while related muscles are kept under control. It seemed impossible to solve this problem, but by training the *S* to cause certain groups of muscles to be more active than others for a designated period, significant tendencies may be observed. The muscles selected for observation were the masseter, which lifts the lower jaw, the temporal, which lifts the lower jaw and draws it somewhat backward, and the pterygoid, which draws the mandibula forward and to one side. These constitute with those of the tongue the chief muscles of mastication. The results of some of these attempts are illustrated in the following tables.

*Accepted for publication November 5, 1928. From the Department of Rural Education.

[1]Cf. N. Krasnogorski, Ueber die Wirkungen mechanischer und chemischer Reizungen verschiedener Teile der Mundhöhle auf die Tätigkeit der Speicheldrüsen bei Kindern, *Jahrb. f. Kinderheilkunde*, 114, 1926, 268-276; I. P. Pavlov, *Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex*, tr. by G. V. Anrep, 1927, pp. 430; A. L. Winsor, Conditions affecting human parotid secretion, *J. Exper. Psychol.*, 11, 1928, 355-363.

The effect on the secretion of the left parotid of opening and closing the mouth a definite amount at designated intervals for successive 5-min. periods is shown in Table I. An increase in muscular tension seems to account for an increase in secretion. During a yawn, which consists largely in the expansion of muscles of the alimentary tract (Condition 4), a very large increase is obtained, with a significant after-discharge from the gland. This accounts for part of the increase in the succeeding period. When voluntary support is superseded by artificial support of the muscles, a definite inhibition is shown.

Further evidence of a direct effect of the muscles upon secretion is obtained when the pterygoid and temporal muscles were activated and the secretion of the right parotid was measured (see Table I). There is, of course, the possibility in these experiments of direct mechanical pressure on the gland affecting the flow. Since gripping the jaws together (Condition 13) shows a definite inhibitory effect even though the mechanical pressure on the gland appears to be greater, it seems hardly probable that this increase (Condition 8 and 9), more than 100%, could be due to mechanical pressure. Some increase, however, can be obtained by massaging the gland. Attention should be called here to the incidental fact that an inhibitory effect is followed by an increased flow.

The effect of various movements on the secretion of both the glands is also shown in Table I. The greatest flow comes from the side in which the muscles are most actively engaged. The muscular activity, rather than pressure on the teeth, as former investigators have suggested, is probably the source of excitation. Part of the flow in this case can be shown to be due to conditioning, and part to the chemicals from the rubber, but when the rubber was chewed constantly over a 3-hr. period so that the chemical and conditioned factors were greatly reduced, the secretion was still 59% of that shown in the table. This would seem to indicate a constant source of stimulation from the muscles of mastication. Condition 19, jaws held to the right as far as possible, shows a direct reaction from the right gland and a greatly reduced or inhibited flow from the left. This is an interesting reaction. It may be due to differences in the tension of the ligaments, and there is also evidence that greater stimulation comes from a series of changes in muscular tension rather than from the continuous tension of the fibers.

In discussing the effects of chewing, Lashley concludes: "(1) Contractions of the muscles of the mandible or changes in the tension of its ligaments do not excite secretion. (2) Pressure on the teeth is, alone, not an effective stimulus."[2] Interpretation of

TABLE I
EFFECT OF MUSCLES OF MASTICATION ON PAROTID SECRETION

Gland		Successive 5-min. periods and conditions of stimulation	Secretion in cc.	
	1.	Erect sitting posture, mouth closed	0.14	
	2.	Opening mouth 4 cm. each 5 sec.	0.20	
Left	3.	Opening mouth 5 cm. each 5 sec.	0.30	
parotid	4.	Holding mouth wide open(S yawned 8 times)	3.03	
	5.	Mouth held wide open (yawning inhibited)	0.60	
	6.	Mouth held open with 4 cm. prop	0.02	
	7.	Erect sitting posture, mouth closed	0.30	
	8.	Lower jaw drawn from left to right, lips closed	0.80	
	9.	Lifting lower jaw and pulling it backward	0.70	
Right	10.	Repeating 7	0.30	
parotid	11.	Head thrown back holding chin high	0.10	
	12.	Repeating 7	0.37	
	13.	Gripping jaws tightly together	0.01	
	14.	Repeating 7	0.35	
			Left	*Right*
	15.	Erect sitting posture	0.25	0.24
	16.	Chewing small rubber ball between left molars	1.39	0.78
Both	17.	Chewing small rubber ball more rapidly on right side	0.82	3.20
	18.	Moving jaws sideways as in 8	0.75	0.90
	19.	Jaw held as far to the right as possible	0.07	0.30
	20.	Mouth wide open (no yawning)	0.60	0.60

the data would seem to indicate that tension of these muscles is one of the sources of stimulation of the salivary gland, and that we have in this mechanism an example of proprioceptive stimulation of glandular secretion. With this as a provisional assumption many of the complex salivary reactions are less perplexing, and if further research substantiates these findings we have here an inviting field for the study of proprioceptive stimulation.

In Table II an attempt is made to illustrate the excitatory effect of greater muscular activity in the swallowing process. Since the pressure of water in the upper part of the esophagus excites the muscular movements necessary for the completion of the act of swallowing, and since water at a moderate temperature

[2]K. S. Lashley, Reflex secretion of the human parotid gland, *J. Exper. Psychol.*, 1, 1916, 463.

does not stimulate the glands directly, the reaction from swallowing 2 cc. of water every 5 sec. was compared with the reaction when 4 cc. were swallowed at the same interval. It was assumed that in the latter case a greater tension of these muscles would be involved. At least the excitatory effect of the new situation is doubled. The effect of the expansion of the stomach as a result of drinking so much water probably accounts for the extra secretion shown in Condition 7 and 8.[3]

TABLE II

EFFECT OF SWALLOWING ON PAROTID SECRETION

Successive 5-min. periods and conditions of stimulation	Secretions in cc.
1. Erect sitting position, mouth closed	0.24
2. Swallowing movements every 5 sec.	0.68
3. Repeated 1	0.27
4. Mouth full of water, no muscular activity	0.30
5. Swallowing 2 cc. of water every 5 sec.	0.43
6. Swallowing 4 cc. of water every 5 sec.	0.93
7. Water in mouth as in 6, but no swallowing	0.51
8. Repeated A	0.46
9. Repeated 1	0.22

Other possible explanations than that of proprioceptive stimulation of these glands need to be considered. One of the first to offer itself is the possibility that all of these reactions are conditioned rather than unconditioned responses. Our experience in studying conditioned salivary responses in man has taught us that any muscular activity involved in the analysis, preparation, or handling of food may become a conditioned stimulus. Even the muscles of the arm become a potential part of the total situation. Hence criteria should be applied to eliminate this possibility, if such can be found.

In the relation between stimulus and reaction there is supposed to be a marked difference between unconditioned and conditioned reactions. The appearance or absence of the unconditioned response is determined mainly by the strength of the stimulus. As the stimulus increases the response increases. In the conditioned reactions on the other hand a weak stimulus may be effective while a stronger stimulus invokes no response whatsoever. A review of the data presented will show a consistent

[3] In a previous report a full stomach was shown to increase the secretion 100%. A. L. Winsor, *op. cit.*, *J. Exper. Psychol.*, 11, 1928, 355-363.

relationship between increase of stimulus and increase in response. The greater the tension of the muscle, the greater the flow of saliva.

Furthermore, conditioned salivary responses as far as our experiments are concerned seem to depend largely upon the state of hunger of the *S*. When the organism is in a state of complacency as far as food is concerned the conditioned salivary responses become very weak or disappear entirely. This is not so true of the reactions under discussion. They are more stable and regular. In fact they have all the characteristics of reflex action. Fatigue interferes only when the stimulating muscle shows fatigue. In a 12-hr. test of the glands no evidence of fatigue was shown, although they secreted many times their weight in saliva. The conditioned response, on the other hand, soon disappears when not reënforced. Even so well established a response as the flow of saliva to the raising of the hand with food in a spoon was temporarily extinguished in one *S* after 45 min. of constant stimulation without reënforcement.

While there appears to be no similar mechanism for comparison, these reactions seem to resemble most the muscular contraction response evoked when the knee extensor is stretched. The receptors for this reflex lie in the fleshy region of the muscle and constitute it a proprioceptive phenomenon. Quick stretches elicit the greatest reactions in these reflexes, as in the motor salivary response. In both cases the source of greatest stimulation is in the change from contraction to relaxation. Both reactions are characterized by recruitment. Under continuous stimulation from an afferent nerve the reaction progressively increases.

As additional evidence for our conclusions we call attention to another more definite source of information too frequently neglected in psychological studies. An examination of the neurological data concerned with the behavior under discussion shows that the connections of the various branches of the trigeminus nerve which supplies the salivary glands have been carefully studied and their relationship to the muscles of mastication noted. The most recent research in this field is an experimental study by Whitehead.[4] The literature reviewed in this report shows that investigators in recent years have found both ascending and descending fibers in the mesencephalic root. The ascending

[4]D. Whitehead, *Central Connections of the Lingual Nerve in the Cat*, Master's Thesis, Cornell University, 1928.

fibers have been traced through the masticator nerve and its branches to the masseter, pterygoid, and temporal muscles. Neurologists conclude that these ascending fibers must carry impulses from these muscles.

Conclusions

Secretion from the parotid gland appears to be the result of a highly integrated nervous action. The nature of this reaction at any one time might be the result of direct or indirect excitation or inhibition whether conditioned or unconditioned. The data in this report point to the possibility of direct proprioceptive stimulation of these glands from the muscles of mastication and swallowing. In addition to the functional evidence presented, attention is called to the fact that recent neurological research has presented conclusive evidence that there are afferent nerve fibers ascending from these muscles to the salivary centers.

PRELIMINARY NOTE ON AFTER-IMAGES FROM STIMULI OF LOW SATURATION AND SHORT DURATION

By M. K. Frehafer, Goucher College

In a recent number of this Journal there appeared a report on the Purkinje after-image by Judd.[1] By courtesy of the Physics Department at Cornell University Judd's apparatus was placed at my disposal; and with some few changes was used for the following experiments.[2]

The after-image series has been extensively studied. In the papers of Fröhlich[3] and of Judd, many references will be found to the previous work on this subject. The positive[4] phases of the series referred to have been designated the Hering image, the Purkinje image, and the third positive image. Every positive phase is preceded and followed by a negative phase. The Hering is of extremely short duration, resembles the primary image in color, and is sometimes observed to be brighter than the primary. The Purkinje is of longer duration, is said to be complementary to the stimulus in hue, and is separated from the Hering by a longer dark interval (*i.e.* negative phase) than separates the Hering from the primary image. The Purkinje in turn is followed by a still longer negative phase than the preceding one, and this by the third positive image, usually similar in color to the primary, of low intensity, and of considerably longer duration than the Purkinje. Following this is the fourth negative, which lingers for a relatively long time. The relative intensities and duration times of the different phases depend on a number of factors, as Fröhlich and others have clearly demonstrated.

Object. The purpose of the present experiment was to find whether a stimulus, approximately white in color, would produce a white Purkinje image. This is one of the problems suggested by Judd, and is of special interest to those who are concerned with the need of a physiological standard white. A tentative standard of this sort is proposed by the experiments of Priest,[5] for which the author was an observer. An object is usually called 'white' if it is whiter than other objects in its vicinity; without comparison of objects one is forced to form a judgment relying on his previous experience with so-called

*Accepted for publication June 2, 1928.

[1] D. B. Judd, A quantitative investigation of the Purkinje after-image, this Journal, 38, 1927, 507-533.

[2] Reported at the October 1926 meeting of the Optical Society of America.

[3] F. W. Fröhlich, Untersuchungen über periodische Nachbilder, *Zsch. f. Sinnesphysiol.*, 52, 1921, 60-88; Über den Einfluss der Hell- und Dunkeladaptation auf den Verlauf der periodischen Nachbilder, *ibid.*, 53, 1921, 79-107; Über die Abhängigkeit der periodischen Nachbilder von der Dauer der Belichtung, *ibid.*, 53, 1921, 108-121.

[4] The term "positive" refers, as it is used in this report, to the brightness of the image relative to that of the background, and not to the hue characteristic of the image.

[5] I. G. Priest, Spectral distribution of energy required to evoke the gray sensation, *Bureau of Standard Scientific Papers*, 1921, no. 417.

white objects. In Priest's method the stimulus was presented alone, many times, with slight color change. *O*'s judgment was influenced by his recollection of the preceding image. In the present experiments one compared the color of the Purkinje with the primary image, the one following the other very rapidly provided the Hering had been suppressed. When the Hering was present it was very fleeting and received very little attention from *O*. Since the Purkinje by its very nature is complementary, there is (ideally) only one color of stimulus which it could match namely, white. The method therefore held the promise of a determination of an absolute rather than a relative white, independent of personal judgment of color and dependent only on *O*'s ability to match two colors seen in rapid succession.

Apparatus. A full description of Judd's apparatus, with diagram, has already been published,[6] and will not be repeated except for the mention of certain modifications and additions. A more intense light source was found necessary in order to arouse a Purkinje image that showed decided color other than violet from a nearly white source, for even a white source if of low intensity will produce a violet or bluish after-image. For this reason a 1000-w. lamp was substituted, with a much larger area of spiral filaments. The vertical length of filament was approximately 1 cm., the width across all filaments 1.3 cm. In order to prevent as much as possible loss of light flux from the larger filaments through the ½ in. apertures of the diaphragms, two steps were necessary: removal of diaphragm E,[7] and a focussing of the filament image farther along, in the plane of G. In the rear of the lamp house was placed a concave mirror, of aperture 18 cm. and focal length 10 cm. such that the lamp filaments were at the center of curvature. This also served to increase the intensity of the beam.

The filters that were used to alter the color of the stimulus were placed in the plane marked by H. These filters were made by the Eastman Kodak Company and were loaned to us by Professor H. E. Howe, who had calibrated them at the Bureau of Standards in terms of color temperature from a standard lamp.

Only one diffusing glass (about 30% transmission) was used in position I, and one 5 cm. diaphragm in position K.

One important addition to the apparatus was found essential, due to the stray light in the room. A house for the observer was constructed with blackened walls and equipped with a ventilating fan. A head rest was also used in place of the eye guide. The little lamps that produced the field illumination were concealed back of the *O*, along the sides of the house, thus illuminating the field indirectly by diffuse reflection from the walls.

Originally it was planned to vary the color temperature of the lamp in rather small steps by changing its voltage, and to keep the brightness of the stimulus the same for all color temperatures by use of a neutral wedge. The latter proved to be undesirable, however, as the necessary reduction in the brightness of the stimulus produced a decrease in saturation of the Purkinje

[6] D. B. Judd, Apparatus for the control of stationary light stimuli, this JOURNAL, 38, 1927, 107-112.

[7] Cf. Judd's diagram, *op. cit.*, 108.

image, to which the *Os* greatly objected. Too great an intensity, on the other hand (this could be produced for yellow stimulus only), had to be avoided, for it served only to blind *O* and render him incapable of making any judgment satisfactory to himself regarding the after-images. With regard to the first point mentioned, eventually it was decided to use only five different color temperatures, which were measured in the following manner. Howe's lamp had been calibrated (color temperature against voltage) both with and without a certain 'daylite' filter; also its color temperature was known at a single voltage for a number of other filters. This standard lamp, with the 'daylite' filter, was color-matched on the photometer bench by means of a Lummer-Brodhun cube, with the 1000-w. lamp and one of the other filters. In this way the calibration curve of the latter combination could be determined. By using the various filters in turn with the two lamps, and extrapolating the curves, the color temperature of the 1000-w. lamp in combination with any filter could be found. The following table, Table I, gives the values so found, with estimated error

TABLE I

SHOWING THE VARIOUS CONDITIONS AND CHARACTERISTICS OF THE STIMULI

Filter Number	Color Temperature °K		Total Transmission of Filter	Brightness in Millilamberts	Color Munsell Notation	Color of Stimulus
	Standard Lamp	1000-watt Lamp				
No filter	2848°	2950 ± 10	1.000	250	YR 7/5	Yellow
1	4234	4100 ± 40	0.286	71	YR 7/2	Less Saturated Yellow
2. Daylite	5550	5300 ± 20	0.165	41	P 1/4	White
3	6100	5460 ± 20	0.184	46	P 1/4	White Unsaturated
4	6680	6040 ± 20	0.147	37	P 3/6	Blue
5	8450	7175 ± 20	0.106	26	—	Blue

due to the use of the extrapolated parts of curves. Column 2 gives the color temperature (degrees absolute) when the filter is used with the standard lamp running at 96.7 volts; column 3 gives the color temperature when the same filter is used with the 1000-w. lamp at 100 volts; and column 4 gives the total transmission of the filter.[8] Column 5 gives the brightness of the stimulus, the measurement of which will be explained in detail later. Column 6 gives, in Munsell notation, the color characteristics of the stimuli. This latter specification is to be taken only as roughly designating the color.

Color temperature specifies the temperature of a 'Black Body' radiating energy with approximately the same spectral distribution as the source which it matches in color. Since this spectral energy distribution for any given temperature may readily be found,[9] an analysis of the source with regard to

[8] These values were also determined by Professor H. E. Howe.

[9] This value may be computed from Planck's equation or it may more easily be obtained from the graphs of Planck's equation that have been plotted on a large scale by M. K. Frehafer and C. Snow (Tables and graphs for facilitating the computation of spectral energy distribution by Planck's formula, *Bureau of Standards Misc. Papers*, 1925, no. 56), and that are obtainable from the Supt. of Documents, Gov. Printing Office, Washington, D. C.

the relative amounts of the various spectral hues is at once available. With this knowledge the reproducibility of the source is assured. Even though the source has not the same spectral energy distribution as that of a black body, this specification is still a very useful one, for when the total radiation of the source is used the eye cannot perceive even large departures from black body distribution.

It is obvious from an inspection of the figures in columns 2 and 3, since they increase at very different rates, that the radiation from one of these lamps, more likely the 1000-w., differed considerably from that of a black body. This was also obvious in some of the temperature determinations. Because of this difficulty some of the color temperatures of column 3 may be in error by as much as 100°. But a still greater source of variation, during the after-image experiments, was the variation in voltage of the 1000-w. lamp, which was connected to the generator. This variation was sometimes as much as 4 volts; and in color temperature this corresponds to a range of nearly 200° at 7000°, and of nearly 100° at 4000°. These errors, however, great as they may appear here, are practically without effect on the results, as will be apparent later.

There is considerable error involved in the brightness values, however, due to this same voltage fluctuation, and the numbers given in column 5 do not pretend to any accuracy whatever. The values given express only the correct order of magnitude. The brightness of the stimulus was measured by means of a Sharp Millar photometer, which was calibrated from a standard lamp. In order to avoid hue differences between the light from the comparison lamp and that from the stimulus, photometric measurements were finally made only on the yellow stimulus (without filter). This value multiplied by the total transmission of the filter for black body radiation of color temperature 2848° K (*i.e.* the numbers of column 4), supplies the results given in column 5. Of course it is evident that some error is also introduced in using transmission values for 2848° K instead of for 2950° K, the actual color temperature of the 1000-w. source.

After investigating the character of the Purkinje image from stimuli of various sizes, brightnesses, and durations, the following type of stimulus was adopted. It was circular, of 5.1 cm. diam., about 10 times as bright in the center as at the periphery. The decrease in brightness from center to periphery is shown in Fig. 1, these values representing the average for a sq. cm. of surface. Unfortunately this falling off in brightness was not regular over the whole patch of light, but some stimuli had a blotchy appearance, which occasioned some *Os* to see more than one hue in the primary image. The diffusing glass on which the stimulus was cast could with advantage have been replaced with a more suitable surface for such a 'graded' stimulus. This was not done, however, partly for lack of time, but principally because the characteristics of the after-image could be discerned well enough even with so imperfect a stimulus.

Observers. There were altogether 10 observers, 6 women and 4 men, including the author, for whom one of the *Os* acted as *E*. Of the 10 *Os*, 5 were red-haired, and 5 were brown-haired. The eyes of the *Os* were of blue, brown, hazel, and gray color. Four of the *Os* were without experience in this work, but all except one had scientific training. The non-scientific *O* was experienced with colors through work in art. Few observations were taken from one of the

inexperienced *Os*, but those taken were of interest because the after-images which were at first called white soon began to take on a 'bluish' appearance. Of the 6 *Os* with previous experience, 3 had gained their experience from Judd's work. So far as appeared from these experiments all had normal vision; but none were actually examined for visual defects. In any extensive series of experiments much more attention should be given to this point.

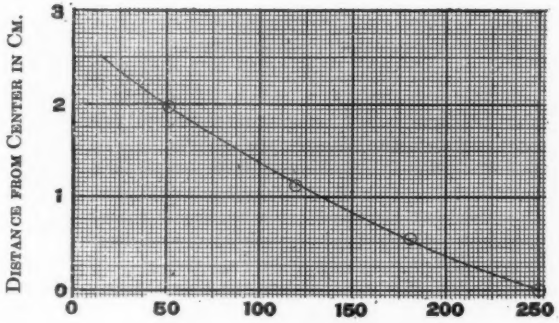


Fig. 1. Decrease in Brightness of Stimulus from Center to Periphery

Procedure. The *O*'s right eye was kept at a distance of 36 cm. from the stimulus, which therefore subtended an angle of 8° on the retina. A small red fixation point (electric light) was focussed on the center of the light patch, and was reduced in intensity by every *O* at his own option, by means of a rheostat. Observations were not begun until after a period of dark adaptation of 10 min. Changes in the after-images due to changes in field illumination were investigated, for brightnesses of the order 0–0.1 ml. Most *Os* preferred a slightly illuminated (about 0.03 ml.) to a totally dark field.

The duration of the stimulus varied from 0.12–0.15 sec. The *Os* were requested not to look at the stimulus until the last after-image had disappeared, for it was found that the saturation of the Purkinje image could be changed by taking the stimulus too rapidly, and it was feared that the hue would also be affected. Observing the stimulus with both eyes open also apparently gave stronger saturation. The fixation light was turned off by some *Os*, as constituting an unnecessary complication.

Results. A graded stimulus has been found to give after-images consisting of a number of bright and dark rings, apparently in motion radially from the less intense to the more intense part of the stimulus.[10] This set of rings was apparent to all *Os* except one. The analysis of the rings in terms of Hering and Purkinje images depended upon their relative brightness, hue, and duration.

[10]Cf. W. McDougall, Young's theory of light and colour vision, *Mind*, 10, 1910, 75, observations 16.

They were followed in the usual way by the third negative and third positive phases. After the primary image and succeeding dark interval, the bright center (brightest part of stimulus) was the first to appear. This became surrounded by dark and bright rings, which rapidly contracted upon the center, seeming to replace it.[11] New rings appeared to come in from the periphery. Some of the *Os* described the motion as due to new rings superseding the old ones. The center was the last part to disappear, partly due to the usual foveal effect of retarded reaction.[12] The Hering image was doubtless in that first flash of the center, for it was colored like the primary, but was so fleeting that it got hardly any attention at all, due to the distracting effect of the moving rings.

With a color temperature of 4100° K (yellow), most of the brown-haired observers obtained a yellow Hering (center) and separated from it by a dark ring, a ring of highly saturated cobalt blue. It seems very probable that this is the Purkinje image. The *Os* who had observed in Judd's experiment had no hesitancy in calling these images by name, without previous instruction. The rings are not easy to observe and describe, for they last only a little more than a second, and only those with very noticeable characteristics receive attention. The other rings are a set of unsaturated light and dark blue circles. In spite of the brightness of the center, it received less attention than the Purkinje phase; hence the *Os* had an excellent opportunity to compare its hue with that of the primary. The Purkinje image of the red-haired *Os* for this stimulus was frequently pronounced purple and lavender.

For color temperature 5300° K and 5460° K, the response as to color of stimulus was white, or cream color, or without hue, or very unsaturated. The corresponding Purkinje after-image was an unsaturated blue, usually designated as bluish-white, the dark circles in this case appearing gray. Occasionally the response was yellow for the Purkinje. Some reported a yellow Hering and blue Purkinje after-image which, so far as one could judge, were complementary. For carefully chosen *Os* this may afford a criterion for a white stimulus, for ordinarily the Hering and Purkinje after-image are not complementary, any more so than are the primary image and the Purkinje. Only the very inexperienced *Os* saw, in their first trials, only white and gray after-images. Again the red-haired *Os* saw purple, pink, and lavender both in place of and in addition to the blue.

The observations do not agree, apparently, with those of Fröhlich, for whom a strong white stimulus yields a white Purkinje after-image. His stimulus may have been of different brightness, however, from those used in this experiment. It may be that a white Purkinje may still be available from a white stimulus of suitable intensity, but not with this moving ring response. Most *Os* objected to a brighter stimulus (for 2950° K—the only one that could be made more intense with this apparatus) as blinding them.

---

[11] A. Samojloff mentions (Zur Kenntniss der nachlaufenden Bilder, *Zsch. f. Psychol.*, 20, 1898, 118-125) images of a similar nature from a peripheral stimulus.

[12] The after-image series described here are much like those that Fröhlich obtained from strong stimulation. Cf. Fröhlich, *op. cit.*, *ibid.*, 53, 1921, 79-107.

On reducing the intensity of the stimulus the details of the flash-images were blurred and confused—a blotchy gray blur of light resulted. This might possibly correspond to Fröhlich's white Purkinje.

For color temperature 6040° K, those to whom the stimulus was definitely blue reported a yellow after-image; those who called the stimulus bluish or greenish, reported only bluish after-images (very unsaturated). Perhaps a higher intensity would have evoked a yellow Purkinje for them. To these *Os* even a stimulus of much higher color temperature, 7175° K, failed to give the yellow image. One O reported the Hering and Purkinje after-images reversed in position, the latter yellow and appearing in the center, while the former, definitely blue, appeared near the edge.

One conclusion to be drawn from these blue stimuli is that, for unsaturated hues, blue is no more effective than any other color for evoking a complementary Purkinje. Previous work has shown that for saturated stimuli blue is apparently the most effective hue. The second conclusion is that this failure of more than half the *Os* to obtain the complementary image from the blue stimulus seems to preclude the possibility of using this method to determine a white stimulus that will evoke a Purkinje similar to itself. For unless this image goes through a white stage as the color temperature of the source is raised from 4100° K, the 'white' stimulus could not be defined, except by particular *Os*, to within 1000° K or more.

One other point worthy of mention with reference to the blue stimulus 6040° K was a garnet-colored ring sometimes appearing near the periphery of the flash-image, occasionally seen by the author. But the auburn-haired *Os* reported pink, pinkish lavender, purple, and violet in the rings, regardless of whether they had pronounced the stimulus yellow, white, or blue. The location of this 'red ring' region on the retina, if there is any such, may have some important bearing on the theory of this subject.

Other results worthy of mention are: (1) After-images were reported from small stimuli, covering 1.2° and 3° respectively of the retina. In the former, covering the fovea centralis, no complementary image was observed for these brightnesses. In the latter, both Hering and Purkinje after-images were observed. (2) A stimulus that showed two distinct hues yielded two different hues in the flash-image.

Summary. With graded stimuli less than 70 ml. in brightness, of short duration, and low saturation, the following results were obtained.

(1) White stimuli 5300-5460° K usually produce blue or purple Purkinje images, rarely white.

(2) Blue stimuli 6040° K usually produce blue Purkinje images, yellow to only a few.

(3) Yellow stimuli 4100° K seem invariably to produce blue Purkinje images, of rather high saturation.

(4) This image is not strictly complementary to the stimulus.

(5) Red-haired *Os* report red (purple, pink, lavender, etc.) in nearly all after-images; brown-haired *Os* rarely reported this hue.

(6) The Purkinje image fails to appear in the fovea centralis, but appears in the parafoveal region.

GRAY AND THE COLOR PYRAMID

By Forrest L. Dimmick and Cyril H. Holt, Hobart College

In a recent article Rich[1] objects to our experimental illustration of the relation of gray to black and white[2] and proceeds to attack the problem by what he terms "a more direct method, namely, the method of constant stimuli." His preliminary results are strongly in favor of our hypothesis of the nature of gray. His disagreement with us is primarily one of method. The problem seems to us to be one in which the psychological approach is more valid and must precede the psychophysical. The question of the psychological character of gray must be settled first by descriptive observations of the criteria in judgments of color relationships rather than by a set of precisely determined limens. The positive indications of Rich's results are open to the challenge that the unique character of gray is implied in his instructions. Consequently we have undertaken to carry a group of naïve *O*s through a long series of color judgments involving all the principal series of the color pyramid, to determine whether the six conventional 'colors' are adequate to a descriptive orientation or whether the *O*s spontaneously include gray as a seventh point.

Preliminary Series

Procedure. The *O*s, three in number (L, N, and R), who served throughout this experiment were undergraduates for whom the theoretical problems involved in the color pyramid had never been raised. Prior to this their experience in matching colors had been only incidental and practical. They sat facing a gray screen in which were two (or three) holes 3 cm. in diam. and 10 cm. apart, through which they viewed the disks of two (or three) color-mixers. By means of a shutter behind the screen the colors were exposed for 2 sec. for every judgment. The four principal hues were presented as standards and the variables approached and receded from them by 5%-steps on both sides in every case. One series in each direction was presented to determine the characteristics of the *O*'s unpracticed judgments of color relationships.

Instructions. In the preliminary series, the *O*s were instructed as follows: "You will be shown two colors and you are to judge whether there is any qualitative resemblance between them. One member of the pair will be varied until the resemblance to the other appears or disappears as the case may be. You will be asked to tell upon what you base your judgments of likeness and difference."

---

*Accepted for publication September 4, 1928.

[1] G. J. Rich, Black and gray in visual theory, this Journal, 37, 1926, 123-128.

[2] F. L. Dimmick, A note on the series of blacks, grays, and whites, *ibid.*, 31, 1920, 301-302.

Results. Table I gives the percentages of the colors in the combination in which the resemblance to a given color appeared or disappeared. For purposes of comparison the chromatic limens of a single trained *O* under similar conditions are appended. The most obvious characteristic in the table is the high percentage of color that was added before a resemblance to that color was noted and, conversely, the large percentage that was still present when the resemblance disappeared. Changes in the original color were noted, however, before the resemblance to the added color was reported. Several peculiarities of judgment are of especial interest to us and their significance was made clear in the *Os*' attempts to describe their bases of judgment. L reported a resemblance to blue in the middle

TABLE I

SHOWING THE PERCENTAGES OF COLOR IN THE DIFFERENT COMBINATIONS AT WHICH THE RESEMBLANCE TO A GIVEN COLOR APPEARED OR DISAPPEARED

R = Hering red; Y = Hering yellow; G = Milton-Bradley green; and B = Hering blue

Standard		Observer L	Observer N	Observer R	Chromatic limens for trained
Blue	Appeared	50Y + 50G 10R + 90B	30G + 70B 20R + 80B	50G + 50B 10R + 90B	G + B 10.3 ± 1.3 R + B 9.0 ± 0.9
	Disappeared	15B + 85G 80B + 20R	60B + 40G 70B + 30R	60B + 40G 90B + 10R	
Red	Appeared	70B + 30R 30Y + 70R	70B + 30R 35Y + 65R	25B + 75R 15Y + 85R	B + R 10.5 ± 0.1 Y + R 10.2 ± 1.4
	Disappeared	15R + 85B 70R + 30Y	20R + 80B 75R + 25Y	70R + 30B 80R + 20Y	
Yellow	Appeared	90R + 10Y 60G + 40Y	80R + 20Y 60G + 40Y	30R + 70Y 40G + 60Y	R + Y 4.6 ± 0.9 G + Y 4.9 ± 1.1
	Disappeared	10Y + 90R 30Y + 70G	15Y + 85R 30Y + 70G	60Y + 40R 30Y + 70G	
Green	Appeared	60Y + 40G 15B + 85G	30Y + 70G 20B + 80G	55Y + 45G 90B + 10G	Y + G 6.6 ± 1.2 B + G 10.5 ± 0.9
	Disappeared	20G + 80Y 25G + 75B	60G + 40Y 70G + 30B	40G + 60Y 25G + 75B	

of the yellow to green series. He had assumed as a matter of course that the mixture of blue and yellow gives green. This was an unstable criterion, however, and blue disappeared in the reverse series before green had been reached. All *Os* tended to call the blue-greens "blue" without any qualification in favor of green. They insisted that they had always called such colors "blue." All *Os* said that they had never thought of orange's resembling red, though they sometimes included it with yellow. Red was generally displaced toward blue; *i.e.* bluish-reds were just "red," not bluish. "Purple" intervened here, making a considerable gap between the disappearance of blue and the appearance of

red, and contrariwise. It appears, then, that so far as our *Os* had any color categories they followed the 'primary' colors red, yellow, and blue, with orange, green, and purple sometimes used on a par with the 'primaries' and sometimes ignored altogether. This follows the usual casual and uncritical treatment of colors.

Main Series

Procedure I. Our problem, now, was to force our *Os* to develop, each for himself, sets of color categories which would be adequate for any problem of color comparison and discrimination that we might set for them. To do this we added to the presentation a second standard which defined further the limits of the series in which the variable moved. Ten series by the method of limits were run through for 16 or 18 combinations of the four hues with each other, with black, and with white.

TABLE II

Showing the Liminal Determinations in Percentages for the Various Colors Used in the Different Combinations

R = Hering red; Y = Hering yellow; G = Milton Bradley green; B = Hering blue; W = Baryta white; and Bk = Velvet black

O	R to Y (%Y)	Y to R(%R)	B to G(%G)	G to B(%B)
L	2.5 ± 0.5	4.8 ± 1.8	18.5 ± 1.2	19.3 ± 3.3
N	4.0 ± 1.2	57.3 ± 1.9	11.5 ± 3.5	9.5 ± 2.8
R			15.0 ± 1.5	14.8 ± 0.9
	G to Y(%Y)	Y to G(%G)	B to R(%R)	R to B(%B)
L			2.5 ± 1.4	2.1 ± 0.5
N	5.2 ± 2.0	5.0 ± 2.0	1.9 ± 0.7	2.8 ± 0.5
R	5.3 ± 0.5	1.0 ± 0.3	2.4 ± 0.6	1.1 ± 0.4
	x to Bk(%Bk)	Bk to x(%x)	x to W(%W)	W to x(%x)
L	6.7 ± 1.0(x=R)	2.5 ± 0.2(x=R)	1.0 ± 0.2(x=Y)	6.8 ± 1.4(x=Y)
N	13.5 ± 1.2(x=Y)	3.8 ± 0.6(x=Y)	5.0 ± 2.0(x=G)	1.5 ± 0.5(x=G)
R	41.8 ± 1.4(x=G)	1.5 ± 0.5(x=G)	1.5 ± 0.5(x=R)	22.5 ± 1.5(x=R)
	R to G(%G)	G to R(%R)	Y to B(%B)	B to Y(%Y)
L	4.8 ± 0.5	17.5 ± 0.2	5.5 ± 0.8	2.6 ± 0.2
N	3.8 ± 1.8	32.0 ± 3.0	4.5 ± 2.0	1.8 ± 0.3
R	15.0 ± 1.0	45.3 ± 2.3	18.5 ± 1.2	7.5 ± 0.5
	Bk to W(%W)	W to Bk(%B)		
L	1.0 ± 0.2	1.1 ± 0.3		
N	1.3 + 0.4	1.1 ± 0.2		
R	1.0 ± 0.5	3.8 ± 1.4		

Instructions. The *Os* were instructed: "You will be shown three colors—two standards and one variable—and you are to judge the relation of the variable to each of the standards. You will be asked to tell upon what you base your judgments."

Results. The rough liminal determinations are recorded in Table II. These values indicate that all *Os* are building up criteria for the comparison of colors, but that during this process there have been several shifts of attitude

under the influence of prior concepts of color relations and of attitudes set up by the experimental conditions. For example, N did not report the presence of a resemblance to red in the yellow-red series until the color was well past the middle orange; L did not recognize the appearance of green in the blue-green series or blue in the green-blue series until a very marked change had taken place; R experienced the same difficulty with green and blue and failed, also, to note the change of white toward red and that of green toward black as readily as he did the others. Reasons for these judgments are apparent in the reports of the *Os* on their bases of judgment. N reported that "the (yellow) disk changed but did not begin to look like the other standard (red) until after it had passed through orange. I always think of orange as a color half way between red and yellow, and colors on the one side are red-orange and those on the other are yellow-orange." L was still of the opinion that "green is made up of blue and yellow" and though "the blue changed" he did not think it could be toward green. Similarly, R in many cases continued to name a color by its original and more insistent component, though "it looked quite different from the standard" until the added color became almost equally insistent. For him "white is still white even though it has a tinge of something else" and "a good green is a dark green."

The values in the cases of the complementary colors are especially important for our thesis. We find that in every case black and white seem to be treated as non-complementaries, *i.e.* white appears in the black, and black in the white with very slight changes from the standard. This might be significant negative evidence were it not for the fact that in 10 cases out of 12 the same results were obtained when the other complementaries were mixed; *i.e.* red became greenish; blue, yellowish; and contrariwise, with a slight decrease in chroma from the beginning standard. This indicates that the *Os* at this stage were judging the first change in the variable and assigning to this change the direction of the second standard without observing such a direction of change. The observations of the *Os* confirm this interpretation. All *Os* stated in similiar language that they "knew that the starting point of the series and the direction of change were given by the two standards" and that, consequently, they "reported the first noticed change in the variable" to be toward the second standard.

Our results thus far demonstrate how effective are our *Os*' ingrained notions about colors in determining their judgments of the presence or absence of color resemblances, and how prone *Os* are to adopt observational attitudes which are inadequate to the problem and which produce misleading results. These attitudes were evident early in the series, but we made no attempt to alter them by instruction. At the end of the series, then, we attempted to change the experimental conditions in such ways as to set up more critical and more adequate attitudes and criteria.

Procedure II. In this procedure we presented two standards with the variable as before but only one of them lay within the series of the variable.

Instructions. The *Os* were instructed; "You will be shown three colors, two of which are standards. The third will be varied serially. You are to judge whether the variable resembles either standard. When it is different

TABLE III

SHOWING FOR EVERY *O* THE COLOR OF THE STANDARDS, THE DIRECTION IN WHICH THE VARIABLES WERE CHANGED, THE LIMENS IN TERMS OF THE PERCENTAGE OF COLOR ADDED, AND THE CRITERIA OF JUDGMENT

	Standards	Variable	*O*	Limens in % added	Criteria of judgment
1.	red and blue	red to yellow	L	1.7 ± 1.4	I noted a change and reported at once.
			N	18.6 ± 6.0	I can see that it changes before I know the direction.
			R	15.3 ± 7.3	More difficult. I had to wait to be sure of the direction. Not like either standard.
2.	blue and yellow	red to yellow	L	1.6 ± 0.7	I reported the first change.
			N	18.8 ± 5.5	I was not sure of the direction at first.
			R	9.5 ± 5.4	I have to wait to tell the direction.
3.	green and yellow	red to green	L	4.7 ± 0.9	I knew it must be green very soon because I saw no yellow.
			N	25.0 ± 3.0	When it stopped being red I called it green.
			R	23.3 ± 5.1	Red faded and I knew green was being added.
4.	red and yellow	black to white	L	19.8 ± 20.0	Usually reported first change. Several times realized I didn't see white at first and waited until I did.
			N	25.3 ± 7.0	Reported white when the variable became lighter and less predominately black.
			R	14.8 ± 6.8	The variable lost its definite blackness but did not become red or yellow.
5.	blue and black	black to white	L	56.8 ± 8.1	I was looking for white this time and didn't find it in the first brightness changes.
			N	23.8 ± 6.0	I reported white when the black became less definite.
			R	13.8 ± 7.2	I called it white when the variable was no longer a good black.
6.	green and yellow	blue to yellow	L	24.3 ± 4.8	I am not as sure of the direction of change as I am of the change but when the blue is gone I know it must be yellow.
			N	49.0 ± 4.3	There is a stage where there is neither blue nor yellow but only gray.
			R	34.5 ± 6.5	The variable became steadily grayer until the blue was gone.

TABLE III (continued)

Standards	Variable	*O*	Limens in % added	Criteria of judgment
7. blue and	white to	L	90.3 ± 3.3	No black appeared in the variable until near the end of the series.
black	black	N	85.5 ± 3.1	The white of the variable becomes first darker, then a good gray. The gray becomes blackish.
		R	77.3 ± 6.8	There is a stage of pure gray where there is no white or black.
8. white and	black to	L	85.3 ± 5.2	The black is not whitelike at first, only grayish.
yellow	white	N	52.5 ± 5.0	White does not appear in the gray until the black is gone.
		R	43.5 ± 9.7	There is a stage which is neither white nor black.
9. yellow and	black to	L	6.8 ± 1.8	The black very soon takes on a rusty or yellowish cast.
white	yellow	N	5.5 ± 2.6	A likeness to the yellow standard appears very soon.
		R	6.0 ± 3.0	I can recognize the change toward yellow very easily.
10. green and	blue to	L	29.3 ± 2.8	When the blue disappears I judge it yellow though it may be only gray.
yellow	yellow	N	43.5 ± 2.8	There is a time when the variable is only gray, then yellow appears.
		R	48.5 ± 7.0	Yellow does not appear until the blue is gone and the variable is gray.
11. black and	white to	L	92.7 ± 1.8	As in series 7.
red	black	N	82.3 ± 2.3	" " " "
		R	82.0 ± 8.5	" " " "
12. white and	black to	L	88.7 ± 4.3	As in series 8
red	white	N	52.8 ± 2.3	" " " "
		R	38.5 ± 7.4	" " " "

you are to indicate the direction of difference. You will be asked to give the bases for your judgments."

Twelve sets of 10 series each were presented to every *O* in various combinations of variables and standards.

Results. The determinations are given in Table III in the order of their presentation. The observational task has been made more exacting in this procedure, since it requires a positive judgment of direction of change which may be forecast by either of the standards. The first effect of this was to

increase the size of the limens and of the MVs. In the earlier series L avoided this effect by the use of secondary criteria which did not fit the problem. In the fourth series he became aware of this inadequacy, and began to shift his attitude so that he made 6 judgments under the first and 4 under the second attitude. This shift is evident in the size of the mean variation and is noted in his reports. From this point on L became more critical of his judgments and distinguished the gray portion of the complementary series as a distinct color which shows no resemblance to any of the six principal colors. The moment of the change of attitude in the other two *Os* is not as obvious, but both found it necessary to distinguish gray from black and white in the same manner as from blue and yellow. The case for gray, then, is just the opposite with our *Os* to that for orange, purple, and the other intermediates. The latter were used at first but soon dropped out, whereas the former was found necessary to an adequate description of the black-gray and gray-white series. This further substantiates our earlier findings and those of Rich, and justifies our treatment of gray as one of the fundamental colors.

Summary

At the beginning of this experiment we found that our naïve *Os* had only incomplete and inadequate categories for the accurate description of colors. Following the errors of practical tradition, they assumed that green is blue-yellow, that gray is black-white, and that orange, purple, and violet are separate color categories. In the course of a long series of judgments of color-likenesses, all *Os* found that the necessary and sufficient color categories or unique colors are red, yellow, green, blue, black, white, and gray. This gives us further justification for treating gray as an unique color. The precise limits of this gray need to be determined by some such method as that suggested by Rich.

THE ILLUSION OF SIZE IN PIN-HOLE VISION

By Norman R. F. Maier, Long Island University

The apparent size of an object considerably decreases when the object is observed through a pin-hole, and the apparent distance between the object and the observer becomes correspondingly greater. The purpose of this investigation was to determine the factors involved in the illusion.[1]

Part I

First the effect upon the illusion of the following conditions was studied; brightness of object, illumination of the room, knowledge of the room, size of the pin-hole, absolute size of the object, and dispersion of light.

Apparatus and procedure. Two round windows in a light-proof box served as the objects of observation. They were covered with milk-glass screens and illuminated from behind. The light box was so divided into two compartments that one window appeared in each. The brightness of each of the object-lights could thus be independently varied, either by adding or subtracting ground-glass screens or by moving the lights within the compartments toward or away from the screens. The size of the object-lights could also be varied at will. The quality of the light was kept virtually constant. The window at the right (called the *primary* light) could be varied in size by rotating near its circumference a disk containing apertures of various sizes, 40, 28, and 20 mm. in diam.

The window to the left (the *comparison* light) had a variable camera diaphragm, with apertures variable between the limits 10 and 40 mm.

The light-box was placed at the end of a series of tables arranged along the center of the room. At the opposite end of this series of tables and 7 m. from the front of the light-box, a large black screen was placed. The screen had two windows 10 cm. square placed side by side and about 20 cm. apart. The window to the right was covered with a large, heavy cardboard disk which could be turned on its axis. Near the circumference of the disk were several small metal plates with pin-holes of various sizes drilled through them. The pin-holes were .03, .04, .05, .07, and .09 in. in diam., as determined by the size of the drill. By rotating this disk pin-holes of various sizes could be presented before the window where a head-rest was placed.

Along the centers of the tables a partition was set up so that the *comparison* light and the large window in the screen were on one side, and the *primary* light and the pin-hole on the other.

The *O* sat behind the large screen. Through the pin-hole at the right the *primary* light could be observed and through the large window at the left the

*Accepted for publication August 15, 1928.

[1]From the Psychological Laboratory of the University of Michigan. The experiments were done during 1925-26 under the direction of Dr. John F. Shepard.

comparison light. In order to make a comparison between the two object-lights the *O* had merely to move his head about 10 inches from one side to the other. The primary light could easily be localized through the pin-hole by the use of the head-rest, which also served to keep constant the distance between the eye and the pin-hole. The judgment was always made with the primary light as the standard. The comparison light was judged equal to it or larger or smaller. *E* varied the comparison light until a perfect match was found. *O* was permitted to make as many observations as he wished before reporting. Both increasing and decreasing variations of the comparison light were made in all cases.

To test *O*'s ability to make these comparisons, readings were taken with the disk containing the pin-holes removed.

The 18 *O*s in this and later parts of the investigation were students, assistants, and instructors in psychology and an instructor in physics.

Results. (1) Individuals differed in the matches made between the primary and comparison lights. The range was 17 to 34 mm. of the comparison light to match a 40 mm. primary light observed through the pin-hole. When the pin-hole was not used, the range was 39 to 41 mm. of the comparison light to match the 40 mm. primary.

(2) For *O*s who had never before been in the room there was a tendency slightly to exaggerate the illusion, *i. e.* the matching comparison light was physically smaller before seeing the room than after seeing it. This increased reduction was as high as 4 m.. for some *O*s, for others only 1 mm. with the 40 mm. primary light.

(3) As the area observed was made smaller, the percentage of reduction by the pin-hole slightly decreased. The diameters of the primary lights used were 40, 28, and 20 mm.

(4) There was a general tendency for a slightly greater reduction for smaller pin-holes, *i.e.* a greater illusion. Five pin-holes were used.

(5) When the primary light was 100 times the brightness of the comparison light it was still seen as smaller. Differences in brightness tended to make the brighter look larger within a range of 2 mm. Beyond this an increase of 6000 times showed little or no difference. The reduction of intensity caused by the pin-hole was never 100 times, so the light observed through the pin-hole was brighter for all readings. When *O* equated both brightness and size, the actual physical difference in brightness was never more than 7.5 to 1.

(6) When both brightness and size were matched by *O* an increase in the intensity of the light observed normally made it appear somewhat larger, a decrease smaller. Within small limits size and brightness were often confused.

(7) In order to test the spread of light beyond the edges of the area, a factor possibly present under normal observation and absent in the pin-hole, two points of light (40 mm. from center to center) instead of an area, were used. Were the spread of the light a factor, it would make the points observed normally appear closer together (or at least not farther), and thus the effect would be opposite to that present in the case of the area. It was found that the pin-hole caused the points to appear closer together to the same extent that it caused the area to appear smaller.

Part II

When both observation lights were reduced in intensity in like proportion, there was less illumination in the room and it was found that the reduction in size caused by observing through the pin-hole had become slightly less. It was then thought that the environment might be a factor, since the brighter light tended slightly to illuminate the room. When observing through the pin-hole the environment was completely removed, whereas under normal observation the slightest amount of illumination might serve as a factor. The arrangement was, therefore, changed for a better control of the surroundings.

Procedure. Two rooms were used. The primary light remained in the room already described and the comparison light was placed in a room to the left. The rooms were joined by large doors so that the second light could be seen by merely turning the head 60° to the left. Thus the rooms formed a sort of 'L,' *O* sitting slightly behind the junction joint. *O* was surrounded by black cloth screens about 1 m. distant so that he was unable to determine the nature of the rooms. The object-lights were seen through windows in the black cloth background so that there were no environmental cues.

The environment for the comparison light was controlled by (1) having the room absolutely dark, (2) stringing from the point of observation to the object-light 8 small lights along the line of vision, lights which could be brightly lighted or dimmed to red spots, and (3) placing a screen in the plane of the comparison light, the screen illuminated from behind as desired. Thus it was possible to remove the environment, to bring the environment between the *O* and the object, or to have the environment on the same plane as the object.

Results. With an environment present for the comparison light, the reduction caused by the pin-hole was slightly greater, and it was greatest when the environment was between the *O* and the object, *i.e.* to make a match it was necessary to have the comparison light smaller with an environment than without. The comparison light always seemed farther away when the environment was removed and yet it required less reduction to match the primary light. Changes in size were also seen as changes in distance. Thus increasing the size of the comparison light made it come closer. The environmental factor was less important in the perception of size than in the perception of distance. It was evidently a minor factor in the pin-hole effect as the greater part of the illusion remained with the environment entirely removed.

Part III

The question was raised as to whether knowledge of the fact of looking through a pin-hole in any way affected the judgment of size.

Procedure. To test this query, *O*s ignorant of the conditions were used. *O* entered the room in the dark, was seated and told to place his head against the head-rest, slightly moving his head against it until a light appeared. The room was absolutely dark and the *O* believed that when his head was placed correctly it caused a contact which brought on the light.

Results. Two *O*s made observations under these conditions and later under normal conditions. The change in technique made no difference in the readings.

Part IV

One can voluntarily change the size of the object observed through the pin-hole by changing one's accommodation. It is possible to get a clear image of an object through a lens, though the lens be out of focus, with the aid of a small aperture. Thus the pin-hole serves to give a clear image no matter what the accommodation is, and the retinal image is made smaller when the lens of the eye is accommodated for some point nearer than the object observed.

To test the importance of this factor a pin-hole was placed near the edge of a piece of tin. *O* was then asked to report concerning the object-light seen through this pin-hole while looking at a light-cross past the edge of the tin. The cross was placed at various distances and *O* was asked to look at it and at the same time to estimate the size of the object-light seen through the pin-hole in relation to that seen normally. The observations were somewhat uncertain. The object-light was still somewhat reduced even when the cross was at the same distance.

Parallel lines were then observed under the same conditions, one end observed through the pin-hole the other end past the edge of the tin. The lines could then be seen to be the same distance apart, both through and past the edge of the tin. That is, the parallel lines looked to be continuous. If the eye was gradually so moved as to see the lines past and then through the pin-hole, the lines were also continuous. On the other hand, when the eye was deliberately accommodated for a near point, so that the ends of the lines seen past the edge became blurred, those seen through the pin-hole were still clear and seemed to move nearer together than the blurred ends.

It is a well-known fact that, in the case of monocular vision, accommodation for a nearer point causes a given object to appear smaller and more distant.[3] But we see in this case of the parallel lines an additional and more extensive reduction in size due to vision through the pin-hole.

By observing through a slit, objects are seen as narrow and tall with the slit vertical, and as wide and short with the slit horizontal. When a round lighted area is observed the same is true; but it is now seen that the sides are blurred in the direction of the slit. This shows that the accommodation is not for the object observed but for some point nearer. The narrow dimension of the slit gives a clear image in spite of the lack of accommodation; whereas the large dimension lengthwise of the slit gives a blur. This blurring was less noticeable when objects rather than lights were observed.

It was also found that as *O* moves closer to the object there is a point of no-reduction in size of the object-light when observed through a pin-hole. This distance is generally about 2 m. from the object.

Helmholtz pointed out that an object observed through a pin-hole is seen as larger if the object is very close.[4] In such a case the accommodation is for objects farther removed than the one observed. The pin-hole prevents blurring and the object is seen as larger because of the large retinal image. *O*s under atropine (and thus without accommodation) were also tested. A far-sighted *O* had little or no reduction in the size of the object-light observed through the

[3] See H. Helmholtz, *Physiological Optics*, Eng. tr., III, 389.
[4] *Op. cit.*, I, 127.

pin-hole. Others with astigmatism and myopia could not get clear enough images with normal observation to make comparisons with the object seen through the pin-hole. As we were limited in *Os*, we were unable at the time to make a complete analysis.

In summary, the facts presented seem to show that, under our conditions, decrease in brightness and elmination of environment through the pin-hole are small factors in the result; but that the 'illusion' is largely due to a reduced retinal image on account of combination of near accommodation with the pin-hole between the object and the dioptric system.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

II. A Variant of the Chessboard Illusion

By Winifred F. Hyde

The chessboard illusion aroused considerable discussion for a period of years following its appearance in the series of pseudoptics arranged by Münsterberg. The chief argument centered about the plausibility of irradiation as the sole or chief factor producing the shift of the central line. Münsterberg, Lehmann, and Pierce favored the irradiation hypothesis, while Benussi and Liel opposed it. Recently Liebmann[1] revived the discussion and concluded on the basis of her results that irradiation probably played an important part but that it could not be the sole cause.

Before the interesting experiments of Liebmann appeared, the writer, not satisfied that irradiation was the cause and thinking that kinaesthetic factors connected with the idea of balance might be a partial element in the explanation, tried the experiment of varying the amount of apparent support offered the upper black squares by the lower ones. Fraser[2] had already combined two sets of squares, thus obtaining a convex or concave central line (Fig. 1). We varied the apparent support gradually from the center out in either direction (Fig. 2) and discovered that the convexity (or concavity) was thereby increased.

In this connection it is interesting to note that the effect of the illusion in both figures is increased if the central part of the pattern is allowed to fall in indirect vision, an observation which has been made on another illusion.[3] The central lines of Figs. 1 and 2 jump considerably if they are shifted back and forth between direct and indirect vision. The convexity seems greater if the central line falls in indirect vision.

The experiment of removing entirely the apparent support of the upper squares was next tried, the idea being that if any weight factor suggested by the black squares were operative, the bases of the black squares would appear to be below the top line of the lower squares. To our surprise we found not only that the upper black squares or rectangles did not descend into the white spaces between the lower squares, but that they actually appeared to have their base lines above the center, producing, particularly where a central line is indicated, a wavy appearance. This waviness is accentuated when a second line is added for comparison and is least apparent when the central line is lacking (Fig 3).

This new form of the illusion appears to lend support to an irradiation hypothesis but no present theory of irradiation seems to explain clearly the curve. Should we not get a zig-zag displacement in this case even more than in the

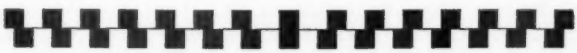
*Accepted for publication July 24, 1928.

[1] A brief summary of several theories advanced may be found in an article by S. Liebmann, Ueber das Verhalten farbiger Formen bei Helligkeitsgleichheit von Figur und Grund, *Psychol. Forsch.*, 9, 1927, 300-353.

[2] James Fraser, A new visual illusion of direction, *Brit. J. Psychol.*, 2, 1909, 307.

[3] S. M. Ritter, The vertical-horizontal illusion, *Psychol. Monog.*, 23, 1916, no. 101, 110.

original illusion? The curve is consistent with the observation that black squares on a white ground sometimes appear to have concave but never convex sides. But granted that irradiation furnishes an adequate explanation, the arguments brought against it in Liebmann's article are in no wise affected by our result.



1

2

3

Using the method followed by Liebmann, we placed a cut-out pattern of squares a little distance in front of a background and then by means of two lights varied the brightness independently for figure and ground. It was thus possible to pass gradually from a pattern of black squares on a white background to white squares on a black background. The conditions were not exact enough to permit measurements, but two interesting results were obtained. (1) The change in the apparent size of the squares is startling. One who had not watched the gradual shifting in brightness would scarcely believe that only one card had been used. The black squares on the white ground appear to be about half as large as the white ones on the black ground. All observers exclaimed at the change. (2) An apparent up-and-down movement like that of a piston is obtained in the upper squares if the rate of change is carefully regulated. Starting with black squares on a white ground and shifting toward white on black, the black squares above appear to move down into the space below them. Of course they become white and move into a black space between white squares. The spatial relationships all change, for the squares becoming white expand before the eyes in an astonishing fashion. The expansion seems to be more within the figure than toward the open spaces of the card; but more controlled observation would be necessary for confirmation. The shifting of brightnesses back and forth between extremes produces the up-and-down piston effect.

APPARATUS

A Readily Adaptable Apparatus for Giving and Recording Stimuli and Responses

By E. E. Brakeman and C. S. Slocombe, Personnel Research Federation

In performing certain experiments, it was necessary to obtain apparatus that would record both onset of stimulus and response on the same paper, and that would also record time to 1/25 sec. without the use of a chronoscope. Recent experimental work was reviewed in the attempt to find such apparatus, but it seemed that there was none that overcame all difficulties. Consequently the apparatus described in this paper was designed and has so far proven quite satisfactory. This apparatus is comprised of three parts, (*1*) a small electric motor (see Fig. 1), (*2*) a recording drum, and (*3*) a device for automatically setting up stimuli.

(*1*) The electric motor (M) is shunt-wound, 110 v., and 1800 r.p.m. It is geared down by means of a worm-driven gear (G) to 3 revolutions per sec. In this set-up there is no clutch between the gear and the rollers which it drives, but it would be advisable to have one. The ratio of the reduction of the speed of the motor is determined by the rate at which the paper must be run through. When this has been decided upon, the size of the driving roller and the ratio of the gear are arranged accordingly. Paper in this set up is run through at the rate of 1 in. per sec., the circumference of the driving roll being 3 in. and making 3 revolutions per sec. With this speed 1/25 in. may be accurately measured. If it were desirable, however, to measure shorter time intervals of, say, fiftieths or hundredths, the paper could be run through at a faster rate. A shaft (T) from this motor is attached to two rollers (R) and (O), roller (R) running the recording apparatus and (O) the apparatus for giving stimuli. The speed of the motor is adjusted to exactly 1 in. per sec. by a resistance in circuit with it.

(*2*) To record responses a roll of paper (P) is mounted on a wooden roller with round grooves in each end into which fit metal points. The degree to which these metal points press against the wood and allow play in the roll is regulated by a screw on one end. The paper (P) is run off around a metal drum (D) and back between rubber rollers (R) mounted on the same wooden support as (D) and (P). Of this pair of rollers one is run by the motor, the other is an idler. The degree to which they press against each other is determined by the thickness of the paper and is regulated by a spring and a screw (S) from the top which presses them together. Just above the drum (D) are copper bronze fingers (F) 5 in. long, on the ends of which are soldered small adjustable Eversharp pencils. These fingers are pulled down by small electromagnets (E) when a current passes. To prevent oscillation in the fingers when they are pulled down and released, a screw (Y), driven through a strip of wood just over the finger, dampens and regulates the freedom of the fingers.

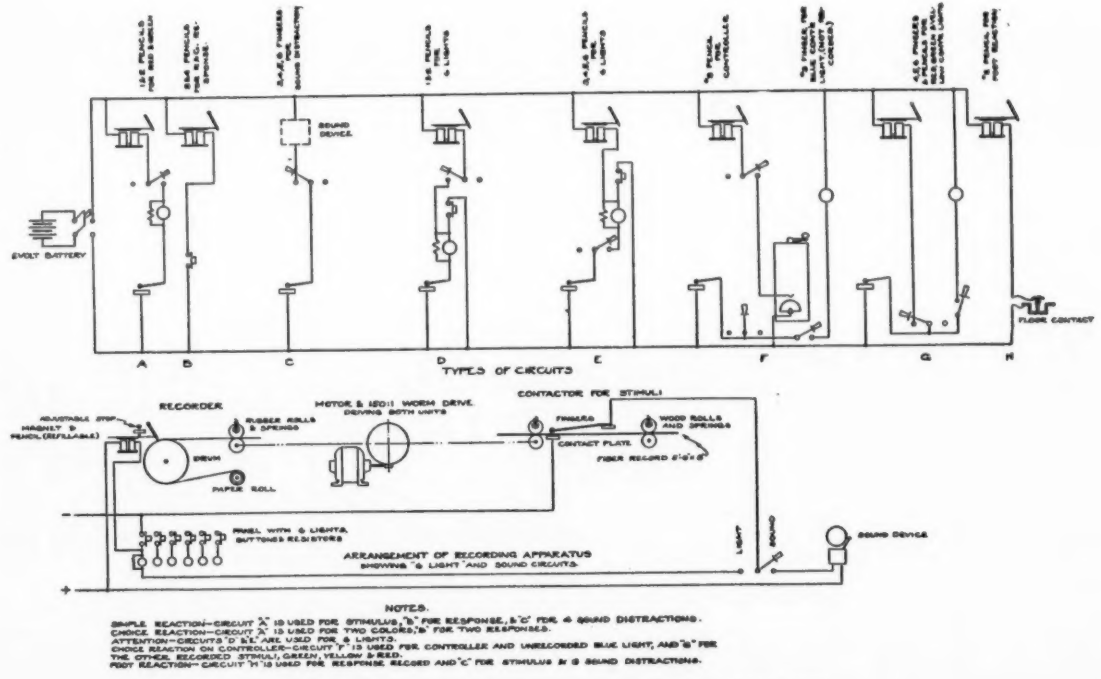

6 VOLT BATTERY
SOUND DEVICE
FLOOR CONTACT
A
B
C
D
E
F
G
H
TYPES OF CIRCUITS
RECORDER
MOTOR & 150:1 WORM DRIVE DRIVING BOTH UNITS
CONTACTOR FOR STIMULI
FINGERS
WOOD ROLLS AND SPRINGS
CONTACT PLATE
FIBER RECORD 5"×6"
RUBBER ROLLS & SPRINGS
DRUM
PAPER ROLL
PANEL WITH 6 LIGHTS, BUTTONS & RESISTORS
LIGHT
SOUND
SOUND DEVICE
ARRANGEMENT OF RECORDING APPARATUS
SHOWING 6 LIGHT AND SOUND CIRCUITS.
NOTES.
SIMPLE REACTION—CIRCUIT 'A' IS USED FOR STIMULUS, 'b' FOR RESPONSE, & 'c' FOR 4 SOUND DISTRACTIONS.
CHOICE REACTION—CIRCUIT 'A' IS USED FOR TWO COLORS; 'b' FOR TWO RESPONSES.
ATTENTION—CIRCUITS 'D' & 'E' ARE USED FOR 6 LIGHTS.
CHOICE REACTION ON CONTROLLER—CIRCUIT 'F' IS USED FOR CONTROLLER AND UNRECORDED BLUE LIGHT, AND 'a' FOR THE OTHER RECORDED STIMULI, GREEN, YELLOW & RED.
FOOT REACTION—CIRCUIT 'H' IS USED FOR RESPONSE RECORD AND 'c' FOR STIMULUS & 6 SOUND DISTRACTIONS.

FIG. 1. DIAGRAM OF STIMULATING AND RECORDING APPARATUS

(3) To give the stimuli, a heavy compressed fiber is run through the two free rollers (O′), passes over the metal plate (A) and under the metal contact finger (X), and runs out through the rollers (O). The pressure and play of these rollers is regulated in the same manner as in rollers (R). The metal contact finger (X), is a flat piece of copper bronze with a small bronze roller ¼ in. in diam. on the tip. In the fiber record slits 5/16 in. long are cut at such intervals as the stimuli are required.

Diagrams of circuits show those for giving and recording stimuli and others for recording responses. Thus when a slit in the fiber record passes over the metal plate (A), the roller drops into it and making contact, closes the circuit (A), for instance, and gives the stimuli. The current passes through (E), pulls down (F), and a dot is made on the record. When *S* responds, the circuit (B) is closed, current passes through (E), (F) is again pulled down and a line is made on the paper. Circuit (B) is 4 volts, that being sufficient to pull down the pencils (F); circuit (A) is 6 volts in order properly to heat the filament in the lamps. They are each run from a storage battery.

For recording, squared paper of 1/25 in. squares and 7 in. in width is used. The distance between the stimulus dot and the beginning of the response line may thus be accurately read in 1/25 in. *i.e.* 1/25 sec., without the use of a chronoscope. The fiber for giving the stimulus is cut 7 in. wide, ruled vertically in half inches and horizontally at intervals at which the slit is cut and stimulus given.

Six pencils and 6 contact fingers were built on this apparatus, enabling 6 sets of stimuli and 6 sets of responses to be given. It is flexibly wired so that any combination of stimuli and responses may be recorded. If for instance when only one light is flashed and the response comes very quickly after the stimulus, onset of stimulus may be recorded on one pencil and that of response on another. In experiments where 6 contact fingers are not used for giving stimuli, the free ones may be used to give distractions.

The apparatus is used at the present time for giving the stimuli and recording the responses in the following experiments. (1) Simple reaction to light stimulus with hand response. (2) Simple reaction to light stimulus with foot response. (3) Choice reaction, simple and reversed. (4) Specific fatigue, and oscillation of attention, by requiring the *S* to respond by appropriate key depression to the lighting of any one of 6 lights in random order and quick succession. (5) Muscular adaptation, by requiring the *S* to move a handle (a street-car power controller) through 240° over 8 notches at a stated speed, with varying resistance to the movement, controlled by a brake-band. (6) Stereoscopic vision, by a modification of Spearman's bioscopic apparatus. Two dark gray glass plates are set up before an evenly illuminated screen; the slow but variable movement of one is controlled by a cam; the *S* is required to move the other to keep it in the same plane. Both (1) and (2) are given with and without distraction by 4 kinds of noises.

These are the uses to which this apparatus has been devoted, but it can be readily seen that any combination of stimulus and response may be used. Either lights or sounds may be used for stimuli or distractions, with either a foot or a finger response. Moreover, it answers the major requirement of automatically giving and recording stimuli and responses in such manner

that the results are readily available for later analysis. It is very convenient and easy to handle, since there is no inking device. The records may be readily revised for giving stimuli in a different way.

A Demonstrational Masson Disk

By Leonard Carmichael, Brown University

The demonstration of the fluctuating bands of the rotating Masson disk is difficult in a large lecture hall if the disk is constructed as it is ordinarily figured in the textbooks of experimental psychology. The difficulty arises because the inner darkest band is still not distinct enough for the persons at the back of the room, and the outer lightest band is not faint enough for those in the front of the room. The demonstration can, however, be made if the disk be larger and the black segments that create the bands, instead of being made all of the same size, diminish in size from the center out, so that the range of difference is greater.

The writer has found the following satisfactory at distances from 6 to 40 ft. The radius of the disk is 20 cm. About a radius as an axis of symmetry a quadrilateral is constructed, tapering toward the periphery of the disk. The base of this quadrilateral is 4 cm. along the diameter perpendicular at the center to the radial axis; the width of the quadrilateral at the extreme periphery of the disk is 2 mm. Within this area nine annular segments are laid out. Each segment is one cm. wide. The segments are separated from each other by spaces of one cm. Each is terminated at the bounding lines of the tapering quadrilateral. The segments on the white cardboard are filled in with black India ink.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Psychology: Its Facts and Principles. By H. L. HOLLINGWORTH. New York, D. Appleton & Co., 1928. Pp. xviii, 539.

This book is evidently intended as a rather serious attempt to re-interpret the main data and principles of psychology and to harmonize the various points of view. In a preface for psychologists, the author explains that the view is not "structuralism," because it deals with activities and processes; that it is not "mere behaviorism," since it does not limit itself "to the account of visible changes" and "insists that aches and pains receive the consideration to which their occurrence entitles them;" and that it is not "associationism," despite certain superficial resemblances, for "it is not 'mere ideas' that are associated but primal events in nature." The author, moreover, holds that he has covered the facts recently emphasized by the *Gestalt* psychology, without special pleading, and has escaped the several vices of the philosophy of mind developed by this school. The book omits any treatment of neuro-physiology. The author regards such data as belonging to another field whose facts have not yet been worked out sufficiently well to show significant correlations with psychological facts. He finds in the psychological facts themselves a unifying principle which everywhere appears, "in whatever psychological activity one begins description." This turns out to be the old principle of redintegration about which Sir William Hamilton, nearly a century ago, was as enthusiastic as the author now is, and which was so fully elaborated by James Mill as the law of association. Here, of course, it is given a modern setting. This principle of redintegrative sequence, we are told, is "at once the criterion, the mechanism, and the dynamics of mind." Once it is accepted, there is left only the matter of referring all sequences to it. This satisfaction with a simple principle, itself not well accounted for, seems to the reviewer to be the weakest point of the book. To this we shall come back later.

The author's psychology is frankly descriptive, "and therefore in the only scientific sense explanatory." This 'therefore' phrase seems to be another wholesale method of settling a point that may well be questioned. Indeed the reader will doubtless be impressed by the author's general tendency in later chapters to fall into a superficial description which really is to a large extent inference from the principle rather than a detailed picture of what really goes on in any case. We shall see. But first, more as to the general point of view. The author first inquires into the question as to what mental processes really are. What are subjective events? He rightly deplores the loose use of the term 'mental,' and limits this term so as to make it practically synonymous with 'redintegrative sequence.' "By a mental process or a redintegrative sequence, then, we shall mean a series of changes in which a partial

antecedent functions appropriately or adequately for a former antecedent of greater complexity" (p. 6). Physical sciences, we are told, are primarily concerned with non-redintegrative sequences, while psychology is interested in sequences which exhibit the redintegrative pattern and also in natural events participating in these sequences. The distinction is admitted to be only a general one, and the various biological sciences come between these extremes to make the transition gradual. The distinction, then, which the author illustrates and elaborates, seems to amount to this: the occurrence of any given 'physical' event, such as the momentary electric oscillations between the pole of a Leyden jar and a metal pencil held near it, is less likely to be accompanied and followed by the group of events in which a similar occurrence of oscillations in a previous 'flash' of lightening had its setting and by which it was followed, than is the presentation of a symbol on a card (before an observer) to be accompanied by the salivation, orientation tendencies, images, memories, pleasures, etc., by which certain features of this symbol had been previously accompanied and followed. Moreover, when the symbol is presented before different individuals, each one reports distinctly different redintegrative patterns. Thus mental events are more variable, too, than physical events. The grouping together of such 'events' as salivation and orientation tendencies with images, memories, and pleasures, does not bother the author at all, for all of these are alike events in the general "continuum of nature." Whether they are conscious events, as pains, sensations, etc., or physical events, makes no real difference, since all alike tend to become aspects of the "public world." 'Subjective' and 'objective' are categories, after all, which refer merely to differences in the consistency or privacy of the events. There is, however, no necessary relation between mental processes and subjective events. These are merely two relatively independent classifications.

A good deal may be said for these relative distinctions, based on redintegrative sequence, consistency or privacy of events, etc., but when the author regards ' a fear,' 'a memory,' 'a sensation,' etc., as in a way coördinate with a flash of lightening, or a rain, he seems to the reviewer to be liable to the use of bad methods as a consequence. It is easy enough to say that all science is 'descriptive,' and that all explanation in a real sense rests on a 'frankly descriptive basis.' But what *is* description? Is it merely sequential enumeration? Or must it, *to be scientific*, not take note of whole circumstances which are not so immediately observable to anyone, such as the differences of potential in connection with the electrical phenomena, the numerous bodily adjustments *of an individual* to certain external circumstances in the case of the fear, the memory, etc. Surely we cannot overlook these conditions and processes as part of the thing to be described. Neither the 'mental' nor the 'physical' events are mere floating incidents. The former are always somehow related to the stimulation and the response adjustments of an organism and are only aspects of the adjustments, as are also nerve structures and currents, inhibitions, facilitations, etc., while the latter are likewise not single, unattached events. The 'behavior of a fear' can certainly not be treated scientifically without consideration of stimulating circumstances, frustrations of physiological processes, sets, etc., in a complex organism which has been a somewhat unitary event in the continuum of nature for many generations. Science has made its

best advances in man's endeavor to *predict* and *control* these processes as the result of experimentation. The book before us suffers not a little from a neglect of this fact. It consequently reads more like an introduction to philosophy than a text-book in science, and the few generalized platitudes of descriptive formulations do not seem to function well when we come to account for such 'events' as the *elimination* of certain response tendencies in learning to the advantage of others. Such topics, and experimental techniques devised to test out various hypotheses generally, are too much neglected. The student using the book is in danger of becoming a philosophizer instead of an experimenter. In justification of this criticism it may be pointed out that many of the experiments which are described and discussed are merely illustrative data and are therefore more or less selected for the convenience of argument. This is, however, to be expected in a book with the purpose of the one before us.

Space does not permit us here to enter the minutiae of redintegrative laws. Any particular experience is always complex; it can never actually recur. What does recur is only "another event in a given class" or series in which the former experience also had membership, as the "same" day of the week. Actually, of course, the day itself is not even the same; but there are elements in common—partial identities— by which the new revives the older event. The response, then, "follows a detail of its former antecedent," the completeness of this revived response depending upon the completeness of the stimulus. But there are various qualifying conditions, such as the prepotency of certain elements of the original experience, the reënforcing or inhibiting effects of different factors operating together or in opposition, the greater readiness of some feature than of others for redintegration, etc. Thus there are many special conditions to consider. These matters, with some applications to special phenomena like synesthesis and to language, take us over nearly one hundred pages of the book. The account of the nature, genesis, and function of language is disappointing, due to the somewhat passive nature of the individual as conceived by the author—the too great emphasis on external stimuli as contrasted with stimuli due to metabolic changes in the organism. If the author were more open to "outside" aid he might have improved his treatment greatly by borrowing something of Jenning's 'spontaneity' in behavior. But he does show well in places how language symbols serve as controls of the behavior of others.

A chapter is given to natural signs and the perception of identity. After considering different views as to what is real with regard to appearances, colors, temporal relations, etc., the author concludes that "natural events exist wherever they are found, by whomever they are observed, and with whatever statistical consistency they are reported, namely, in the realm of nature. The *seen* pencil is the *real* pencil, although the visual features do not by any means exhaust it" (p. 115). The treatment of perception of identity is clear and logical and includes various experimental data, but it seems to the reviewer to suffer from the same over-passivity of the individual reactor (in as much as he enters at all) that was mentioned in connection with the treatment of language. The experiments, moreover, are used merely to illustrate the theoretical contention. "Thought and Things: The Nature of Ideas" is the heading of a long chapter in which the ideas or thoughts are treated as occurrences in the

same realm as the things for which they stand. The erroneous notion of an 'inner world' as different from the 'outer world' is attributed to the fact (1) that adults can think of things in the world, however remote, without maps and other objects to represent these things, and (2) that the materials employed in this inner world seem to be usable by only one person. This duality, however, clears up with an analysis of the origin of these two conditions. Events are ideas only when they function as dynamic cues or antecedents and their power is attributable to their natural history, their association with the power of things for which they stand.

Two chapters are given to spatial and social perception. In these occurs a good deal of material derived from experiment, including the interpretation of facial expressions in photographs. Much space in later chapters is devoted to learning and retention (or memory), but the reviewer feels that the account of learning is in some respects superficial and that it avoids some of the most important problems in this field (indeed in the whole field of psychology), probably because they cannot be made to fit into the simple redintegrative formula. How will the author deal with learning which is contrary to both frequency and recency expectations, with intensity controlled? How will the redintegrative formula serve to explain the tensions and the peculiar narrowing of the subject's range of orientation in certain cases of confusion, and how will it function in processes of reconstructive thinking? A (mostly theoretical) chapter on imagination, dreams, and invention leads up to motivation, which is treated, in accordance with the author's general formula, as synonymous with the "antecedent, stimulus, or cue which initiates the process." Inner 'drives'—such as hunger pangs—are given as the basis of certain persisting motivations, but there is not sufficient recognition of the fact that several of these drives may be wholly outside of the individual's awareness. 'Motives' are persistent stimuli which when organized into a plan by means of symbols become 'purposes.'

A peculiar result of the author's point of view brings such topics as reflexes, habits and instincts under the head "Consequents of Mental Activity." Only small space is given to instincts, but the treatment is good and profits by the recent discussions of this topic. The great adaptability of the innate mechanisms in the human organism is emphasized. Despite the attention paid to motives, drives, cravings, etc., in connection with motivation, a full chapter of considerable length is later given to cravings, bodily feelings, and emotions, treated here, however, from a larger—though still rather introspective—point of view, with no inclusion of experimental data. In order not to identify Watson's 'rage,' 'fear,' and 'love,' as observed in infants, with the more precisely differentiated emotions of adult life, the author proposes for them the terms 'resistance,' 'startle,' and 'content,' adding a fourth (supposedly necessary) emotion—'gloom.' Later chapters cover the topics of reasoning, attention, voluntary action and decision, the senses, and intelligence. In view of our limited space the contributions in these chapters do not justify an analysis here. Appendix I is devoted to lists of classifications of reflexes, instincts, emotions, etc. by different psychologists; Appendix II, to rather extensive projects, questions and exercises for the several chapters, which are sure to be useful to instructors; and Appendix III contains references for supplementary readings, also arranged by chapters.

The general impression left on the reviewer's mind is that this volume is too theoretical and mechanical, and does not contain enough data (though it is rather too large) for a good general introduction to psychology. Its point of view is not that which emphasizes enough the experimental possibilities and methods of psychology; it leaves the student devoid of empirical means of working out his own problems. It will certainly not interest the great majority of college students and lead them into psychology as a career. For an advanced, critical course it will be more suitable as a general guide, though most instructors will desire to supplement it by the use of other works which this volume represents too superficially. As to its contributions to psychology, the reviewer cannot feel that it has solved the vexed problems of duality of 'mind' and 'matter' or made any one real contribution either in method or in content. The point of view is, however, clearly presented; definitions are well formulated; and many useful suggestions as to treatment of particular problems and data are afforded. It is impossible in a review to do entire justice to the author's complete presentation of the facts and principles of psychology, and to evaluate the possible effects of this book on the fallacious dualism of more carelessly written texts. The book merits wide and careful consideration.

J. P.

A State Educational System at Work. By M. V. O'Shea. Madison, Wis., The Bernard B. Jones Fund, 1927. Pp. xvi, 368.

This volume is a supplement to the author's earlier *Public Education in Mississippi*, and deals especially with the intellectual status and the educational progress of pupils in the elementary and high schools and freshmen in the colleges, both public and private, of Mississippi. It is a part of the reports on the educational survey of the State of Mississippi directed by the author, and carries, besides numerous tables and illustrative figures on the data obtained in the measurement work and in classifications, recommendations as to desirable modifications of educational procedure in the state. The book is very simply written and copiously illustrated so as to reach the general reader in the state. In this respect it appears to be a successful piece of work. The book might be read with profit by citizens of other states who should have a better insight into the procedure of surveys of educational systems, particularly as to the testing technique when carried out on a large scale. The psychologist and educator can in a short time get a fair idea of its contents from the tables alone.

The book as a popularizer of psychological and educational work is unfortunately not without faults, some of them serious. For example, a wrong conception of the "probable error" is given. This measure is defined and illustrated as follows: "One measure which indicates the limits of possible modification of our conclusion is known as the 'probable error,' or P.E. To illustrate: The median score of three thousand sixth grade white pupils in a Southern state is 99, say. We compute the P.E. and find it to be 0.8. This means that if every sixth grade white child in the state would be tested, the median score would not be changed from 99 by more than 0.8 either way—it would not be more than 99.8 nor less than 98.2, with the probability that it would fall at

99" (48 f.). This is, of course, entirely wrong. Passing over several conclusions relative to education, which we should be inclined to question, we may note that the results of the extensive mental testing with various group tests are handled most uncritically. Scores from various standard group tests at different grade levels are converted into IQs and handled as if all these IQs were alike and genuine. No attempt is made to determine the amount of discrepancies in results from different tests, or the effects of the lack of opportunities as compared with children from whom the norms are taken. The reviewer suspects, from his own experience with tests in the South, that chronological ages were very inadequately obtained, especially in the case of negro children. It is well known that the IQs from different group tests cannot be directly compared without considerable error, and also that some of them reflect much more than do others the effects of educational opportunities.

The survey finds the negro children much retarded in their school progress as compared with the whites, and a much more rapid elimination of negroes from school with advance of grades is also shown. "The difference between white and negro children in mental capacity is a real difference and is large." The difference varies throughout the twelve grades on large numbers from, roughly, 24 to 55 times its P.E. (p. 134). This is in conformity with what other investigators have found in the South, except that the difference here is probably larger than usually found. The size of the difference is, unfortunately, not easy to obtain, except in the IQs. On a total of 23,555 white children distributed through grades 1 to 12, a median IQ of 95.3 ± 0.09 was obtained, while on a total of 4948 negro children similarly distributed the median IQ was 75.0 ± 0.17, as the reviewer has figured these constants from data in tables on page 118 for whites and on page 132 for negroes. On the basis of this difference the author recommends a different educational regimen for the two races in the elementary and secondary schools, and also in the colleges (p. 5). This is probably a little premature in view of the many yet unsolved problems before the psychologist in race testing. A differentiated program for both races so that each child, irrespective of race, could advance regularly in the sort of work for which he is most fit, would doubtless be a much better recommendation. It would also meet the demands of any established race differences as these differences become determined. In view of the greatly inferior opportunities of the negro children in the Southern states generally, this recommendation by the author (based too much on mere scores without due consideration of relative opportunities) will doubtless have an unfortunate effect on the negroes, so far as future opportunities both in Mississippi and in other states are concerned.

The reviewer believes that the author's stress on advancement by mental rather than by chronological age is unfortunate, as it tends to justify the absurd practice of giving all children in each race the same training regardless of their several aptitudes and general abilities. If in the present survey attention had been given to more critical evaluations of the factors making for efficiency in mental tests—consideration of speed factors and sets for speed, of training in motivation, etc.—the author would doubtless have found a better explanation than he gives of the fact that the Mississippi children make a relatively good showing in the so-called achievement quotients, and that pupils in small towns

with inferior educational opportunities especially stand high in this regard. An under-rating of intelligence scores, of course, will tend to bring about just such results. The conclusion that "pupils in rural districts may be less distracted from school work than are pupils in large communities, and that they may possess greater interest in school work and greater endurance, industry, and ambition to succeed, since they have rather meager educational opportunities as compared with pupils in larger places" would doubtless have been different if more thorough investigations even on relatively few cases had been made, and if the author had been better acquainted with the methods and the needed precautions in mental testing.

The usual sex differences were found: boys were a few months older than girls in all the grades of the elementary and the high school, except the first grade and the last two years of the high school, the difference in most cases being statistically reliable. The IQ of the girls was found to be higher than that of the boys, the difference being more than four times its probable error in most of the grades. The difference is reversed (and favors the men) in colleges but it is not reliable, statistically. The author believes that when training and ages are equalized, however, the sex differences between men and women will not be reliable. The tests used in the colleges—American Council Psychological Examination—undoubtedly show environmental and training effects. The differences found in the grades are in all probability due to the greater maturity in development of girls than of boys. The intelligence tests used in the grades and high schools in comparing races and sexes were Pintner-Cunningham Primary Mental Test for grades 1 and 2; National Intelligence Tests, Scale A, Form 1, for grades 3 to 8, inclusive; and Terman Group Tests of Mental Ability for the high school students. Certainly no serious student of race differences would be tempted to put much reliance on any of these tests as indicators of native differences, under the usual conditions of different training and cultural backgrounds.

J. P.

The Solving of Problem-Situations by Pre-School Children: An Analysis. By AUGUSTA ALPERT. New York, Teachers College Contributions to Education No. 323, Columbia University, 1928. Pp. viii, 69.

The author of this study has adapted Köhler's well-known problems for chimpanzees to children. Experimenting with forty-four children, whose ages ranged from 19 to 49 months, she devised two series of tests, as follows:

Series I. (A) A toy was suspended from the ceiling which reached within four feet of the floor. Five feet away diagonally to the right was placed a hollow block, nine by twelve by fifteen inches, which could be used as a foot-stool for reaching the toy. In (B) a chair was substituted for the block. In (C) the objective was placed on a shelf, four feet above the floor, with the block present for use as a foot-stool. (D) was like (A), except that the objective was hung higher, and could be reached only if the block were placed on end. In (E) the objective was hung still higher, and could be reached only after the block had been placed on top of a larger box which was now placed in the room.

Series II. (A) The child was placed in a play-pen, the objective being outside, and a stick about a yard long inside. In (B) a toy broom replaced the stick. In (C) the stick was now placed outside the pen, but it could be reached by a shorter stick which was inside the pen. In (D), in order to reach the objective two halves of a fishing rod must be joined together and used as a single stick.

Only four of the forty-four children made no attempt to solve the problems. Direct responses on the part of those who did make the attempt were about twice as frequent as indirect responses. There was practically no correlation between success in the solution of these problems and the mental ages of the children, thus confirming a general observation that social and emotional immaturity are the most likely reasons for failure.

An analysis of the behavior shows four types of response: (1) A primitive response, like reaching with the hand. (2) A random response which seems to be largely attributable to resentment at being thwarted. (3) Exploration and elimination of unsuccessful means. (4) An immediate solution which may include a brief primitive response.

After the first success the primitive response becomes infrequent. There were fewer random responses in Series I than there were in Series II. The subject does not adhere to one type of response but varies with the needs of the situation. Exploration and elimination appeared most frequently of all, with primitive responses a close second, while random responses were relatively infrequent.

The analysis of the solutions indicates "insight" rather than "a fortuitous constellation of elements." "If this whole question is regarded for the time being outside the setting of theoretical implications, the assumption that the solutions of the present problem-situations were accompanied by insight is not only warranted but inevitable and even obvious. What is not obvious is how insight evolved, and this appears to the writer to be the crux of the problem" (p. 38). The solutions are classified, first, as solutions with immediate insight; second, as solutions with gradual insight, which may be either partial or complete; and, third, as solutions with sudden insight which may mature during the exposure or between exposures to the same problem.

With respect to transfer, the author finds it to be almost perfect in Series I from A to B and C, and in Series II from A to B. Sixty out of seventy-one possible solutions in these experiments were immediate and are accountable in this way. Of the eleven cases showing absence of immediate solution in I-B, I-C, and II-B, six solved the key-situation with only a partial insight, and the other five can be adequately explained on other grounds, which makes "a fairly strong case for transfer as a criterion of insight" (p. 45).

There were no "chance solutions," although it may be said "that chance may be instrumental in bringing about an optimum constellation of the elements of a problem-situation, and that this constellation may or may not arouse insight. But unless insight is aroused, the most favorable constellation may go astray and the problem remain unsolved" (p. 50).

There follows a comparative study of the behavior of children and apes in these tests, and an analysis of the various causes for failure to solve the problems.

Cornell University R. M. OGDEN

Psychology and the Soldier. By F. C. BARTLETT. Cambridge, Cambridge University Press, 1927. Pp. viii, 224.

This book is a selection from certain lectures on the relations of psychology to military problems, given by the author in Cambridge. The treatment is somewhat general, dealing with principles, and is too abstract for most immediately practical purposes. Its three parts deal respectively with Choosing and Training the Recruit; Leadership, Discipline and Morale; and Mental Disorders of Warfare. Emphasis is put on the changes in psychology from speculation about the contents of the mind to the study of how people behave and why they behave as they do. Tests of visual acuity, the American army intelligence tests, and certain tests of special abilities are discussed as to nature and purpose. A few well known and experimentally established principles of learning are discussed in relation to the acquirement of bodily skill. It is pointed out that periods showing plateaus in learning should not be allowed to discourage one, and the Bryan-Harter interpretation of the plateau is offered. Persistent and prolonged repetition is discouraged as being related to becoming "stale," and the benefits of properly distributed practice are urged. An experiment on typewriting is described which led to the conclusion that stress on speed with neglect of accuracy at first and stress on accuracy to the neglect of speed, come out, when the stresses are shifted later to the neglected aspects of practice in each case, very much alike. The conclusion was supported that "a high quality of work can be obtained finally without insisting upon a high quality constantly throughout the learning process," but it is, of course, not regarded as final and of universal application. This ready change from accuracy to speed, and contrariwise from speed to accuracy, is held to be important. Valuable suggestions for motivation are offered, and the author advises against unvarying uniformity in practices. He suggests careful study of the effects of factors like shiftings, different rhythms, durations of practice, and holds that "nobody has as yet produced unequivocal evidence for the occurrence of direct mental fatigue" (p. 90). Mental fatigue shows itself indirectly in various nervous and mental disturbances, which are considered in the latter part of the book. Group psychology, in relation to morale, discipline and punishment, is handled less experimentally, of course, than the foregoing topics, but nevertheless with judgment and discrimination; and the author indicates various means of building up morale and shows that officers who are responsible for the conduct of court-martial should have some acquaintance with modern psychology. An interesting and valuable discussion of three types of leaders is given—the *institutional* type of leader, who must emphasize his rank and authority, the *dominant* type, and the *persuasive* type. "The institutional leader *must* remain aloof; the dominant leader *may* remain aloof; the persuasive leader *dare not* remain aloof" (p. 148). The last part of the book takes up (1) "the course of a soldier's entirely normal reactions to modern warfare," (2) the origin and nature of the symptoms of conversion hysteria and anxiety neurosis, two of the greatest classes of mental troubles developing in conditions of modern warfare, and (3) methods of treatment. These discussions are based on general experiences in the World War. It is, of course, hard to say just what the normal soldier's experiences are and there are differences of opinion concerning many

details as to the on-set and real nature of the mental troubles. The author is not unaware of these facts and shows the caution of the scholar. Throughout the book he makes the reader realize that we are on new ground in which only a beginning of exploration has been made, and he emphasizes the need of adequate facilities, supplied by military authorities, for the prosecution of psychological research in the preparation of military careers.

J. P.

The Scientific Habit of Thought. By FREDERICK BARRY. New York, Columbia University Press, 1927. Pp. xiii, 358.

In four chapters—Science and the Sciences, The Nature of Fact, The Elements of Theory, and Scientific Humanism: A Corollary—the author attempts, apparently both as chemist and as philosopher, "to impart to the people at large the living spirit which animates (the scientist's) labor, the instinctive [sic] philosophy which guides it, or the methods which make it effective." The book is the substance of casual notes "taken down at intervals during four years of collegiate teaching in the history of sciences." While to the onlooker science is knowledge, to the scientist as worker it is activity; the spectators see the game, but the players live it. The particular methods of ascertaining facts vary in the different sciences with the contents investigated, but the general attitude of the investigators is similar. The results of one scientific activity provide the methods of another, and "the wonder of knowledge is not the richness of its present revelations but the limitless possibilities of its self-fertilizing growth." All "tough-minded" persons are scientific but not scientists, the latter only being those who make the search for new knowledge a profession. Scientists are the most reticent and inconspicuous, in the popular view, of the designers of our civilization. They are objective, detached, imperturbable, coldly and even recklessly rational. To them it matters little whether facts are beautiful or ugly, useful or not; they are not to be selected for private ends—such handling of facts is not science. But these are their attitudes only as scientists. In fact "all people are tender-minded part of the time, the larger number a greater part of the time, many all the time; this is their common nature, and in this their brotherhood, which is real, consists." The characteristic traits of the tough-minded are only adaptations, liable to revert and show instabilities. Scientists appear to "find their greatest satisfaction not in the indulgence of their emotions or aspirations but in the prosecution of affairs." The gratification of curiosity is stressed too much by the author as the motivation in science. Curiosity is often but a superficial drive.

The valuable discussions of the nature of fact and the elements of theory are well worth reading. The general attitude taken by the author is pragmatic, and reveals indebtedness to writers like John Dewey. The handling is unfortunately, due to its abstract and historical nature and the limitations of space, beyond the grasp of most general readers even of college grade; and the illustrations are too narrowly confined to the science of chemistry. The chapter on scientific humanism is hardly up to the level of the earlier part of the book. It considers the rôle of science in modern culture, and also methods of the in-

corporation of science and the genuine scientific attitude into the curricula and of stimulating generally among intelligent men and women an interest in scientific matters.

The author disregards too much the enormous individual differences in intellectual capacity, and therefore the inability, whatever the opportunities for the development of good *habits*, of a very large percentage of citizens to participate in any real way in the scientific activities of our time. An appreciation of this fact, established in many psychological researches, modifies considerably the problems involved, and would give the author more cause for concern. His position in respect to this fact of individual differences is too much that of the theorizer.

J. P.

Sex Development in Apes. By H. C. BINGHAM. Baltimore. Comp. Psychol. Monog. 5, 1928, (no. 1), 1-165.

The author has had the opportunity of gathering first-hand data concerning the sexual manifestations of primates whose development has reached the late stages of infancy, childhood, adolescence, and maturity; hence he is able to give a serial and cross-sectional view of their sexual responses in various stages of functional integrity and biological serviceability. Preceding the data on sexual behavior, there is a section devoted to the morphological characteristics of the accessory reproductive apparatus of apes. By critical comment and quotations from the literature, the author calls attention to the piecemeal nature of our information on the external generative organs of the primates and indicates the limitations of generalizations given without knowledge of species, ages, and individual variations in the animals observed. In this connection he emphasizes the importance of regarding his own summarizations and interpretations as strictly limited to the observations reported.

The second section of the paper deals in part with the literature of sexual behavior in monkeys and apes and in part with observed incidents of copulation in a variety of adult primates of the Abreu colony of Havana. With necessary reservations, it is suggested that the copulatory behavior of monkeys tends toward greater precision and stereotypy than that of adult apes. Complete proof of this statement cannot be given, however, because the genetic aspects of sexual activities in the species considered have not been adequately studied. The fact of greater precision and lesser variation in copulatory acts of common laboratory and domestic animals seems to be an established fact, hence it would not be surprising to find that in the monkeys, which are lower in point of development than the apes, the same condition obtains. Presence of the observer did not serve as a restraining influence on the sexual practices of primates at any age. Indeed in certain instances it served to stimulate sexual activity in previously quiescent animals. Nothing suggestive of shame or secretiveness was associated with the sexual acts, which leads to the question as to whether such emotional manifestations in man are not primarily conditioned by environmental influences rather than innately organized. Through observations and reports from keepers the fact was established that copulation between pairs of compatible individuals is very frequent, several times daily, and that its oc-

currence bears no specific relation to the phenomenon of oestrus. Likewise the state of pregnancy did not restrain the female from giving herself to sexual indulgence. These conditions of sexual receptivity in the female clearly differentiate the primates from other mammals below the monkeys in the development scale and bring them more nearly in line with the human species.

The third main division of the monograph deals with the rise of sexual activities in young chimpanzees from what was thought to be the first clear-cut sexual response to responses sufficiently complete to permit of intromission. The data warrant the generalization that sexual acts of a variable nature gradually become organized into the copulatory response as seen in the adults and that the animal's previous experience plays a prominent rôle in what may be seen in any given cross-section of sexual behavior. The elements of sexual acts are frequently observed in other integrated series of responses that at the moment have no sexual focus whatever. In short, there is no copulatory pattern of activities which appears *de novo*. In this respect it is of interest to note that observations on the apes conform more nearly to the alleged facts concerning the human species than to the established facts for many, *but not all*, domestic and laboratory animals. Those who believe that apes are capable of various degrees of ideational behavior will find no obstacle in the way of assuming that young apes engage in sexual practices long before the age of puberty primarily for the purpose of the sensual gratification they afford. To speak of sexual play in this pre-pubertal period is no less appropriate than to speak of the many other manifestations or playful activities indulged in by these young animals.

Section IV describes many forms of sexual behavior which serve no reproductive end. The responses are generally known in the literature on man as perversions or aberrations of the sexual libido. Young and adult apes exhibited various forms of homosexual adjustments, masturbatory acts, and exhibitionistic tendencies. Again it may be said that in these respects apes conform to the alleged facts concerning man and the observed facts concerning many of the lower animals.

The value and importance of this painstaking study rests less upon the novelty of the author's contribution to the descriptive and genetic aspects of sexual phenomena, than upon the fact that the animals under consideration are on a plane of mental and physical development more closely approaching that of man than any of the laboratory or domestic animals heretofore subjected to detailed study. For that reason it suggests more adequately the nature and order of development of sexual activities in man than any of the studies previously reported.

Institute for Juvenile Research, Chicago, Illinois CALVIN P. STONE

Infancy and Human Growth. By ARNOLD GESELL. New York, The Macmillan Co., 1928. Pp. xvii, 418.

In this book Dr. Gesell has brought together material from a number of his previous books and articles and has added some account of further experimental work now in progress at the Yale Psycho-Clinic. As in his earlier work, *Mental Growth of the Pre-School Child*, the viewpoint taken is that individual

differences in mental ability are apparent even during the first few weeks of life, and that these differences are maintained at approximately the same relative level from early infancy to maturity.

Following an introductory chapter, the book is divided into three parts. Part I deals with observational methods. A full description of the plan and equipment of the Psycho-Clinic and the Guidance Nursery is presented, together with many photographs. An extremely valuable feature of this part has to do with the method of constructing an observational screen for one-way vision, and a photographic dome with electrically controlled cameras which provides for the exact synchronization of simultaneous photographs taken from different angles (described also in this JOURNAL, 40, 1928, 126-128).

In Chapter V the method of studying developmental changes by simultaneous comparison of the behavior of two children of different ages, which was first described in his earlier work, is again brought into play. The paired subjects range in age from 1 to 12 mo., and each successive comparison is made between children who differ in age by only a single month. Chapter VI summarizes the various normative items which have been found typical of children at each monthly age-level from 1 to 12 mo., and thereafter by 3 mo. intervals to 30 months. According to his usual custom, the author groups these items under four main heads: motor, language, adaptive, and personal-social behavior.

Part II includes a group of special genetic studies of infant behavior. In Chapter VII Gesell discusses the form of the mental growth curve, and the consistency of the "developmental quotients" (identical in derivation with the familiar "intelligence quotient"), obtained through repeated examinations according to his methods. This chapter is open to definite criticism. The consistency of the reported findings for the same children from examination to examination is almost unbelievable until one notes the statement (p. 141) that "the repeated examinations were made by the same examiners, as it was our purpose to build up a cumulative familiarity with the child. In each instance, therefore, the last examination was brought into comparison with the results of the preceding examination. A new series of ratings were made and *the data on the previous examination were used critically in the weighting of the succeeding examination*" (italics mine). When the highly subjective nature of many of the individual normative items is taken into consideration, one can hardly avoid wondering how much of the reported consistency of results would remain if the usual scientific requirements of independent observation and judgment had been adhered to.

The remainder of Part II is given up to a series of studies on special aspects of mental growth. The case study method is used throughout. Normal, retarded, and accelerated children are compared as to development during the period of infancy. There are a number of interesting reports on cases showing atypical glandular and nutritional conditions. A chapter on twinning includes an account of the mental development of two cases of hemi-hypertrophy, which the author, in common with many others, regards as a form of minimal twinning. A description of the mental development of premature and postmature infants presents some data to indicate that, as Scammon has demonstrated in regard to physical growth, the normal development cycle is but little affected by the incident of birth. Conception, rather than birth, should be regarded as the zero point in computing indices of development.

Part III, "The Significance of Infancy", presents some comparative data on human and animal infancy, discusses the relative influence of hereditary and environmental factors, the possibilities of prediction, and the social importance of early prediction, particularly in cases of adoption.

Taken as a whole, the book is highly suggestive. Many problems are pointed out, and certain outstanding trends of mental growth during the period of infancy are described. Those who have been inclined to regret that the emphasis upon end-results presented both in this and the preceding book has led to rather meager descriptions of the methods by which these results have been arrrived at, will be particularly gratified at the promise of a detailed manual giving complete descriptions of procedure together with diagnostic norms. We shall look forward to the publication of this manual with much interest.

Institute of Child Welfare
University of Minnesota FLORENCE L. GOODENOUGH

Psychology in Personal Selling. By A. J. SNOW. Chicago, A. W. Shaw Co., 1926. Pp. xvi, 620.

In late years there has been a great emphasis by certain writers on the 'psychology' of salesmanship. Evidently because the seller is better organized and financially more able to reward such services than are the individual buyers, the writers are more ready to help him with his aspect of the double process of selling and buying. We do not yet see books on how to avoid buying what is of relatively little value to one, or even on the psychology of individual buying. While there is no denying that advertizing and selling play important rôles in the general education of the public and in the standardizing of products, it is equally undeniable that there are many so-called psychologies of these functions which are no credit to psychology; indeed, they are mostly books by popular writers who know very little of psychology and who care less for scientific standards. The author of this book has formerly attempted in his *Psychology of Business Relations* to emphasize the fact that " a good sales situation has three elements, (a) a real need, want, or desire, (b) a solution, and (c) satisfaction."

The present book is divided into four parts: (1) Psychology of Personal Selling; (2) The Salesman; (3) The Sale; (4) Selection and Training of Salesmen. The author first attempts to give an outline of reflexes and their modifications; motives to action, to fear, to fight, to conceal, to shelter, to own, to assert oneself, to be submissive, to know, to imitate etc.; individual differences; perception; habit; ideation; etc. Other conventional topics come in under "the sale." The author follows the fashion and is a 'denier' of instincts (of the incredible armchair type of 'instincts') and yet his treatment of the "desires, wants, needs" etc. just given shows that the departure from the conventional is only superficial. While much of this conventional treatment seems to the reviewer somewhat valueless and far-fetched (probably for any book on psychology), considerable objective data valuable to the business man are included, though unfortunately they are not always highly reliable. The high intercorrelations among personal traits as estimated, for example, by friends and

acquaintances show only that there is a generally high agreement in the *use of certain terms* applied to people, but not that these judgments actually evaluate certain objectively determinable traits. Recent studies have warned us against the customary ill-founded conclusions on this point, and have revealed the immense complexity and ambiguity of various so-called personality traits. Yet there is undoubtedly something there to study when we can do it thoroughly. The author recognizes the unsoundness of much work in this field, and cautions the reader against uncritical acceptance of the view that personality traits can readily be determined. Despite the fairness and relative objectivity of the author in the treatment of personal selling, one cannot but feel that the development of tactics for "putting over" the sale is one-sided and should allow more room and stimulus for rational consideration on the part of the buyer in view of relative needs and values to him. The book will doubtless aid, possibly very much, workers in certain commercial fields who are mostly ignorant of scientific methods of psychology. It is written by a regularly trained psychologist.

J. P.

Index Psychoanalyticus. Compiled by JOHN RICKMAN. London, Hogarth Press and Institute of Psychoanalysis, 1928. Pp. 276.

This is a listing of 4739 titles on psychoanalytic and allied subjects, for the years 1893 to 1926. They are arranged chronologically under each author's name and the authors' names appear alphabetically. Most of the titles are, as might be expected, in English and German but there are also titles in nine other European languages. The list includes translations and reprintings and with many of the titles there are further references to abstracts.

There can be no need here for amplification upon the value of indexes and bibliographies. Every reader of this journal knows their value full well. Because of such values and because of much personal labor with the confusing literature on psychoanalysis the reviewer welcomed this volume almost joyously, but as he works with it he has been disappointed almost to the point of annoyance. As a piece of bibliographic work it has many defects. References to journals are often incomplete. Sometimes the number of pages is given and sometimes not; sometimes only the initial page appears, and one cannot even be sure of finding that. Such omissions are sometimes, curiously enough, followed by a very complete reference to an abstract. Book titles appear with and without an indication of the number of pages. There are, moreover, certainly some unfortunate title omissions. Without even turning to his own bibliographies, the reviewer proceeded to look for a half dozen titles, which he could recall off-hand, some of which were published in the very journals which this compilation pretends to cover, and was unable to find them. Worst of all there is no classification by topics. Of course that would have added greatly to the labor and to the cost, but the reviewer believes that a classified index of psychoanalytic literature is the greatest single need of those who would work in this field. Defective as they are, a classification of these titles, after the manner of the thirteenth number of the *Psychological Abstracts*, would be enormously valuable.

The system adopted of referring to a journal by a single letter, *e.g.* M for *British Journal of Medical Psychology*, and Y for *Jahrbuch für psychoanalytische Forschungen*, while doubtless justifiable on an economic basis is nevertheless very trying.

University of Oregon EDMUND S. CONKLIN

The Union List of Serials in the Libraries of the United States and Canada. Edited by WINIFRED GREGORY. New York, H. W. Wilson Co., 1927. Pp. 1588.

This *Union List of Serials* is a bound volume embodying the result of years of planning and coöperative work by members of the American Library Association. It gives, by means of simple abbreviations and symbols, the distribution of over 75,000 serials in 225 of the most important libraries in the United States and Canada. A 'serial' is defined as "a publication not issued by a government agency, appearing at regular or stated periods of less than a year, and including articles on various subjects." In this volume, therefore, are included the various psychological periodicals of the world, in as much as they will be found in the coöperating libraries. Changes that have been made in the name of each journal and the dates of such changes are given. The listing of psychological periodicals in the different languages seems to the reviewer to be rather complete, though he has, of course, not had time to make a systematic search in this *Union List* for all such journals. The volume is an excellent and scholarly beginning in a useful line of coöperative activity by the American Library Association. The Association was aided in the compilation of this list by a grant of $10,000 from the Laura Spelman Rockefeller Memorial. The book is very helpful and will save much time to departments and institutions which are completing their files as to the volumes in different serials, or which desire to add other series of periodicals, because the *List* indicates conveniently the existing volumes in any series, the dates of beginning and of ending (if the periodical has discontinued), together with any divisions, combinations of different periodicals, and modifications of titles that have been made. It also enables the researcher to locate readily in the nearest library the serials which he may need that are not available in the library of his own institution.

J. P.

Educational Psychology: An Objective Study. By PETER SANDIFORD. New York, Longmans, Green & Co., 1928. Pp. xix, 406.

By some, Psychology has been defined facetiously as that which the psychologist studies. An examination of texts in Educational Psychology may lead to a similar definition for that special field. The author of this volume, however, has defined the field for his purposes by stating explicitly that he limits his treatment to "the objective and verifiable data of carefully controlled experiments." This he has done, for the most part, after a preliminary and elementary discussion of "How the Human Body Works." The major portion of the book is devoted to some aspects of the problem of learning, presented chiefly through reports of experimental studies, with quotations and abstracts here and there of theories of learning. No educational psychologist would deny

the value of objective studies designed to clarify the learning process, but the presentation of these studies alone seems inadequate in dealing with learning. The educational psychologist cannot escape the necessity of formulating principles; for these principles are essential if educational theory and practice are to profit from psychological investigations. This is particularly true, it seems, with respect to the stages of learning, the learning process, and the patterns of learning. In this connection, the author is content to quote Thorndike's laws (use, disuse, satisfactions, bonds, etc.), although it has been pointed out by others that these are perhaps factors affecting learning rather than laws of the nature of the learning process itself.

If, however, one accepts the author's premise that the subject matter of Educational Psychology should be restricted to the presentation of purely objective and verifiable data, it will be found that the volume has fulfilled its purpose by the richness of its material. The details and data of many experiments are cited with reference to such problems as conditioned stimuli and conditioned reactions, laws of and improvement in learning, permanence of improvement, fatigue in learning, and transfer of training. There are also brief chapters dealing with the measurement of intelligence and with the measurement of achievement by means of the new type, or objective, test. Throughout these chapters, it seems, it has been the author's purpose chiefly to indicate the nature of the experimental data, rather than to develop principles from them.

Cornell University FRANK S. FREEMAN

An Approach to the Psychology of Religion. By J. CYRIL FLOWER. New York, Harcourt, Brace & Co., 1927. Pp. xi, 242.

The author's fundamental thesis is that religious phenomena are connected with original tendencies "not directly, as immediate, distorted or sublimated expressions of their operations either singly or in cooperation, but indirectly, as the result of their failure to function in an ever-increasing discrimination of features in the environment," a discrimination altogether in excess of man's innate tendencies for direct response, and which gives rise to what is designated as the "frustration experience." He assumes that "beyond situations" give rise to phantasies and images which are projected upon them in two main ways: (a) phantasy as a means of practical control, (b) phantasy as a means of partial substitution. The first gives rise to scientific knowledge, the second to religious faith. A distinction is made between imaginative interpretation and phantastic distortion. One is practical and adaptive, the other impractical and mal-adaptive. Herein lies the essential difference between religion and insanity.

Later chapters make applications of the above theory, as an explanatory principle, to the religion of the Winnebago Indians, the peyote cult among the Winnebago Indians, and the religious experiences of George Fox. The source material is purposely selected to represent religious experience at different cultural levels, and in each instance the author implies that a satisfactory explanation has been achieved. Some of the salient features of the conversion experience are explained as frustration phenomena, and as the most frequent

particular instances of "the first hand religious response." The author criticizes the views of Jung, Martin, etc. as being unduly depreciatory of religion. Religious phantasy is not merely an escape from a reality which is displeasing, it is a construction about a reality which is baffling. It is difficult to evaluate empirically in a reliable manner the interesting hypotheses of the author.

University of Georgia JAMES E. GREENE

The Child Guidance Clinic and the Community. By R. P. TRUITT, L. G. LOWREY, C. W. HOFFMAN, W. L. CONNOR, E. TAYLOR, and F. R. KENDEL. New York, The Commonwealth Fund Division of Publications, 1928. Pp. 106.

This little book consists of a group of papers written from a number of points of view describing the function of the child guidance clinic in the community. Dr. Truitt presents a brief outline of the history of these clinics, and dwells particularly on their function as centers for community education in mental health. Dr. Lowrey outlines the general plan of organization, and describes in some detail the various kinds of services rendered. Judge Hoffman points out the relationship of the clinic to the juvenile court, and gives a summary of the results of preventive work with juvenile delinquents in Cincinnati, together with several case histories. Mr. Connor points out the services which the clinic may render to the public schools. Miss Taylor deals with its services to children's agencies, and Mrs. Kendel dwells upon its functions as a means of bringing about better understanding between parents and children in the home.

A foreword by Graham Romeyn Taylor points out the changing conception as to the function of these clinics. "Attention was naturally focussed first on the value of the clinic itself, with its coördinated service of psychiatrists, psychologists, and social workers, to the children concerned." "More recently, however, members of the various professional groups and laymen interested in this service to children have become aware that its effectiveness depends largely upon community understanding of the clinic's function, and the proper adjustment of the relationships between the clinic and the various agencies with which it coöperates.

Institute of Child Welfare
University of Minnesota FLORENCE L. GOODENOUGH

The Nature of Conduct. By PERCIVAL M. SYMONDS. New York, The Macmillan Company, 1928. Pp. 346.

This volume is an interesting and suggestive discussion of a significant educational problem. Conduct, according to the author, is "that overt behavior of a human being which involves an issue respecting himself, that issue making a difference in his satisfaction with life." Stated more specifically, the writer is interested in the essential psychological nature and the development of such complex forms of reactions as courage, truthfulness, etc.

Phenomena of this sort are "the habits and skills with which we meet life situations." They are not to be regarded as unitary traits but each is typically a group of relatively independent habits. Also, the component habits are re-

sponses to definite stimuli and such loose unity as may exist is due primarily to a stimulus factor common to the several stimulating situations.

There are several very effective features in the book. A chapter on the social anthropology of norms of conduct and another on the place of reasoning in conduct are excellent. Also, through the extensive citation of experimental literature, several important aspects of the general problem are made clear.

The author undertakes to treat his subject from the point of view of behaviorism. Although he presents the behavioristic concepts very effectively, he does not hold to them with perfect consistency. A discussion of the rôle of the affective processes involves introspective data almost exclusively. The concept of purpose is also introduced in the final chapter.

Dr. Symonds is seriously confused about one point. He says that the problem of ideo-motor activity is, "Do ideas have control over overt conduct?"

This volume can hardly serve as the sole text of a course in elementary educational psychology because of its restricted scope. As supplementary reading, it will serve to correct a host of misconceptions.

University of North Carolina ENGLISH BAGBY

The Cave Man's Legacy. By E. HANBURY HANKIN. New York, E. P. Dutton and Co., 1928. Pp. 180.

Somewhere between the anthropoids and the most primitive human life known there must have been a stage in our ancestry when men were extraordinary brutal. During the known history of man this brutality has slowly subsided along with the development of morality, but it remains in us as an instinctive tendency which is often manifested. This is the gist of the author's thesis. The scope of his topic is notable. From the first chapter in which there is an animated presentation of his own ability to communicate with monkeys, the author leads his reader through primitive pugnacity, murder as a habit, cannibalism, evolution of the human mind, the origin of morality, events of the French revolution, the Paris Commune and the American revolution.

One would think that the author had never heard of John Watson and the critical literature of instinct theory. He speaks glibly of the instinct to kiss, sexual jealousy as an inheritance from animal ancestors, love of dancing, the property instinct, the stranger-hating instinct (the instinctive desire to kill a stranger on sight, of which race prejudice is a vestigial form), instinctive cruelty, a natural tendency to sympathize with aspirations for home rule, and perhaps others. There is, curiously, no use made of G. Stanley Hall's more scholarly presentation of a very similar thesis concerning an assumed exterminating tendency in primitive man. Such a book as this is very likely to give the reading public a distorted notion of what psychologists are thinking.

University of Oregon EDMUND S. CONKLIN

The Psychology of Youth. By E. J. SWIFT. New York, Charles Scribner's Sons, 1927. Pp. xi, 342.

This volume represents a new edition of Professor Swift's book, *Youth and the Race*, published in 1912. The publication of this revised edition is very timely in view of the rapid increase of freedom gained by the adolescent youth since

the World War. Concerning the 'Flaming Youth' of today, Professor Swift says, "Perhaps the 'revolt' is no greater than in the past, but with a wider range of opportunities it assumes more alarming proportions." After presenting many up-to-date examples of adolescent difficulties a solution similar to that of the earlier book is offered. The author concludes: "Definite rules of action for tapping the reservoirs of racial and social energy cannot be given. It is largely a matter of personality and tact in dealing with children. But the first requirement in the teacher is to remove the coating of pedagogical tradition. Then he is ready to absorb modern ideas. If such a teacher determines to find a plan which will create in the children the same enthusiasm for school work as they have for their own activities, he will know when he has succeeded."

Princeton University — W. S. HULIN

Orleans Algebra Prognosis Test. By J. B. and J. S. ORLEANS. Yonkers, N. Y. The World Book Co., 1928. Pp. 14.

As the title indicates, this test is intended to predict a student's chances for successful work in high school algebra. It examines the student's ability to handle situations such as he must meet in the study of algebra, and also his knowledge of arithmetic and other information necessary for learning algebra by methods now in common use, and his skill in applying such information. Character traits—such as attention, persistence, interest—are regarded as being an important factor in algebra achievement, for some of the students used in the standardization process were graded on the New York Rating Scale for School Habits. The multiple correlation for the prognosis test and the rating scale, on the one hand, with achievement test scores on the other, was 0.82. For the prognosis test and achievement scores the multiple correlation was 0.71. The achievement scores were taken after one term in the study of algebra. The authors do not state whether the prognosis test is to be regarded as a criterion of the quality of work at any level in secondary school algebra, or whether it is valuable chiefly or only for the most elementary levels.

Cornell University — FRANK S. FREEMAN

Objective Tests. By J. S. ORLEANS and G. A. SEALY. Yonkers, N. Y., World Book Co., 1928. Pp. x, 373.

This book presents a plan for the local construction and use of objective school tests for instructional, administrative, and supervisory ends. It is also intended for the use of teachers and school officers in a course of training. The subject is presented through a detailed description of a testing program carried out in one of the supervisory districts of Lewis County, N. Y. The volume deals but very little with the general theory of standardized tests; it is devoted rather to the problem of local needs and their possible satisfaction through the coöperative work of teachers and school officers. The authors have included sample tests in a number of subjects and sample record sheets for the assistance of those who will use the book as a guide. The authors have been careful in the presentation of their material and modest in their claims for the tests and programs which they have outlined.

Cornell University — FRANK S. FREEMAN

NOTES AND DISCUSSIONS

Energy, Engines, and the Engineer

A Critique of C. Spearman*

Professor Spearman's *The Abilities of Men*[1] can hardly fail to be reckoned among the few masterpieces of expository writing in psychology. There is not an obscure sentence in it, and by generous use of summaries and recapitulations the essential points are driven home. His long-known doctrine of '*g*,' the general factor in intellectual abilities, is now fully before us. Its proof rests upon the 'tetrad difference' formula, the argument being that given four tests each measuring a different mental function, if the difference between the product of any two correlations and the product of the other two correlations approaches zero, the functions tested must involve a single general factor and four specific factors. This condition Spearman finds fulfilled by a large variety of mental tests. He supplies the essential need of a formula for the probable error of the tetrad difference, and measures the amount of the general factor involved in any mental function tested, by an ingenious use of the formula for partial correlation.

Discussion of the statistical validity of these methods may be left to the experts in that field. The present paper is concerned rather with Spearman's conclusions as to the nature of the factors which he discovers through analysis of a large mass of experimental material.

These factors are the following. In the field of cognitive processes three general factors are revealed by the tetrad difference method. The first is Spearman's original general ability, '*g*,' which appears in tests involving 'eduction,' or the cognition of relations and correlates; for example, in the opposites test and in various reasoning tests. The second general factor is relative absence of perseveration, as shown by the ease with which a person can pass from one task to another. The third is oscillation of efficiency, as seen in rivalry, fluctuations in the limen, and continuous mental work. There are also four less general or group factors, manifested in certain groups of tests; these are logical, mechanical, psychological (apparently meaning ability to interpret other minds, to deal with human nature), and arithmetical. In the field of conative processes, Spearman accepts as a general factor Webb's '*w*;'[2] self-control, or purposive consistency, "a tendency to take pains in the present for the sake of gains in the future" (p. 354).

Spearman's interpretation of these factors relates them to mental energy, to the engines through which that energy works, and to a mysterious engineer. In the first place, the general factor *g* is supposed to be the quantity of mental

---

*Read at the meeting of the American Psychological Association, Columbia University, December 27, 1928.

[1] C. Spearman, *The Abilities of Men*, 1927.

[2] E. Webb, Character and intelligence, *Brit. J. Psychol., Mon. Suppl.*, 1, 1915, no. 3, 1-99.

energy possessed by an individual. Perseveration represents the degree of inertia affecting this energy, and oscillation indicates the rapidity of its fatigue and recovery.

In the second place, the group factors and the specific factors are held to depend not on the supply of mental energy, but on the efficiency of various particular engines, peripheral or central; thus the specific musical ability may depend on peculiarities of the ear and of the auditory region of the cortex.

Thirdly, as for the conative general factor, character or self-control, "some of us," says Spearman, "may be inclined to take yet another step and think that where energy and engines operate there must furthermore exist an engineer. And this requirement also seems to be met, namely, in the conative law—as specially manifested in *w*" (p. 415).

The present writer objects to these interpretations in two particulars. First, to the vitalistic assumption of an engineer. Secondly, to the assumption of a mental energy whose laws do not harmonize with those of nervous energy.

As regards the first point, the hypothesis of an engineer who is not himself an engine may, I think, be regarded as a product of that tendency to mysticism in biological speculation which seems to have followed the war as a kind of fatigue phenomenon: whenever it is not perfectly simple to explain a biological fact by ordinary physical laws, the easy way is to conceive the activity of a super-physical being. I have elsewhere[3] suggested a possible mechanistic basis for directed thinking and that self-control which constitutes Webb's *w* and Spearman's engineer, and will not discuss this matter further here. The main concern of the present paper is with the second point, the assumption of a mental energy whose laws do not harmonize with those of nervous energy. I wish to show that this lack of harmony arises wholly from Spearman's error in conceiving his general cognitive factor, *g*, as due to *quantity* of mental energy, instead of thinking of it as dependent on a certain structure of the central cognitive *engine*, the cortex.

To begin with, let us see how Spearman conceives the relation of mental energy to nervous energy. He would like to think of the two as identical. He quotes as expressing "a promising idea" Myers's statement, "I see no reason why we should not identify central nervous energy with mental energy" (p. 407). But for the present he admits that the two forms of energy do not coincide. The conception of mental energy is, he says, "incomparably less definite than that of the physical energy which serves the physiologists so well." As for the relation between the two things, this, he continues, is for the present "buried in complete obscurity."

Just what the incompatibilities between the two types of energy are Professor Spearman nowhere concisely states; we can, however, discover them by a careful consideration of what he has to tell us about mental energy.

The most striking incompatibility seems to lie in the fact that individuals with most nervous energy, so far as we know anything about nervous energy, seem not to be those individuals who have the greatest quantity of mental energy, of *g*, general cognitive ability. For example, a large stock of nervous energy is put at the disposal of the organism by the action of the endocrine

[3] M. F. Washburn, Purposive action, *Science*, n.s. 67, 1928, 24-27.

glands. But the person whose endocrine glands function most energetically, the emotional person, is not apt to be the person in whom we find the most cognitive ability. Moreover, it is often precisely when the endocrine glands are most vigorously acting that the processes which manifest *g*, the processes of relational or eductive thinking, go out of business.

Shall it be said that the person with most mental energy is the person in whom the greatest proportion of available energy discharges not in the thalamic region, to produce the manifestations of emotion, but in the cortex, to produce thought processes? But not only does *g* fail to correlate with quantity of energy released by the endocrine system; we have no assurance that it correlates with the quantity of energy used in cortical processes. Memory, at least in its higher types, is in all probability a cortical function; and one of Spearman's most striking discoveries by the tetrad difference method is that *g* does not correlate either with retention or with recall. Reproductive processes involve no *g*. While memory weakens with age, he finds no corresponding diminution in *g*. It is in the processes that Spearman calls *eductive*, the cognition of relations and correlatives, and in these alone, that *g* is shown. But to assert that eduction requires more mental energy than does reproduction is merely to beg the question.

Surely the peculiar nature of *g* which causes it to be exercised in eduction but not in reproduction constitutes a disharmony between mental and nervous energy that may be removed by merely supposing that *g* correlates not with quantity of mental energy but with a peculiarity of the cortex as engine. On the mental side the difference between reproduction and eduction is that reproduction need not involve analysis of situations, while eduction always does; to discover relations we must analyze. What this analysis means on the cortical side we do not know. A motor theory would have it involve the power of making many combinations and recombinations of many different small and delicate movements. Perhaps "delicacy of response" is as good a name as any for that trait of the cortex as an engine which must characterize the brain of a person in whom Spearman's *g* is large. It is not quantity of nervous energy, which may be larger in a stupid but emotional person; not even quantity of nervous energy in cortical processes, for this may for all we know be as high in the person whose superiority lies in memory as it is in the thinker; but a certain capacity for detail reactions rather than mass reactions, a structure innate in the cortex of such individuals as possess keen reasoning power, the 'electric aptitude for seizing analogies,' and lacking in the cortex of those dull persons who reproduce their previous responses *en masse* and without recombinations.

Spearman himself suggested in 1906[4] 'plasticity' or 'plastic function' of the nervous system as the correlative of *g*, and mentions the fact in *The Abilities of Men* (p. 91), apparently as a matter of merely historical interest. Why does he now adopt the so much less appropriate and so much more mysterious concept of quantity of mental energy? Curiously enough, his strongest reason

[4]F. Krueger und C. Spearman, Die Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten, *Zeit. f. Psychol.*, 44, 1907, 50-117.

appears when looked at more carefully to indicate that *g* rests *not* on quantity of energy, but on plasticity or something like it, of structure in the cortex, the general engine.

The reason lies in what he calls the law of universal mental competition or of span: "every mind tends to keep its total simultaneous output constant in quantity, however varying in quality" (p. 259). There is, in other words, a sharp limit to the number of mental processes that can occur simultaneously. When a group of beans is exposed to view for a very short time, "*any* two may both be seen clearly . . . so long as not too many others are seen clearly also. . . . Similarly, one can easily follow any two instruments in an orchestra" (p. 114). It is this limitation of output, suggesting as it does the law of the conservation of physical energy, which seems to be the fact that influences Spearman to think of *g* as quantity of mental energy rather than as due to a peculiarity of structure in the engine.

But obviously in order to conclude from the limitation of mental span to the limitation of mental energy as shown in *g*, it must be shown that size of mental span as such correlates with *g*. One is astonished to find from Spearman's data that it does not so correlate *as such* at all. The following are his data. In tachistoscopic experiments with verbal material, children previously estimated as more intelligent showed larger spans; but as Spearman says, this may have been because the more intelligent children had learned to read better. When non-verbal material was used, the correlations with *g* were low. Only one research, that of McQueen,[5] in which the observer was required to perform two mental operations at once, is regarded by Spearman as trustworthy; here the correlations with *g* were satisfactory. But Spearman himself points out that in such cases there is often rapid alternation between the tasks instead of simultaneous performance, and—here is the point at issue—that when they are simultaneously performed, it is usually or at least "much more naturally" *when there is cognition of the relations* between the two tasks (pages 264-5). Obviously this means that mental span as such does not correlate with *g* at all; the observer with the greatest span is not the observer with most *g unless the processes involve eduction*. It is not mental span but eduction, the cognition of relations, with which *g* correlates; the law of mental span then can furnish no evidence that *g* is quantity of mental energy. The only trustworthy experiments point rather to the conclusion that *g* is an innate characteristic of the general engine, the cortex, by virtue of which analysis and response to relations become possible.

As for the other two general factors which the tetrad difference formula discovers in cognitive processes, Professor Spearman suggests that one, oscillation in efficiency, is due to a peculiarity of the central energy, namely, the speed with which it recovers from fatigue. "Such an account," he says, "of the way in which fatigue is manifested by the general energy furnishes the needed complement to the preceding chapter, where an account was given of how fatigue is manifested by the specific *engines*" (page 327, italics mine). The other general cognitive factor, the perseverative tendency, he explains as due to the

---

[5] E. N. McQueen, The distribution of intelligence, *Brit. J. Psychol., Mon. Suppl.*, 1, 1917, no. 5.

degree of inertia belonging to the individual's mental energy, which, he says, "is shown in slowness of shifting from one set of engines to another." But surely fatigue and inertia belong to engines rather than to energy?

In comparison with the solid bulk of Spearman's achievement in analyzing and measuring the energies of men, it may seem an unimportant matter to criticize his interpretation of *g*. My interest in it arises from two sources: first, the obvious wrongness of the interpretation, and secondly, a feeling that the assumption of a mental energy whose laws are inconsistent with those of physical energy arises from the same mystical tendency that assumes the control of an engineer. We all inherit an engine which secures for each of us a certain available supply of physical energy; this engine is our metabolic apparatus, including the endocrine glands. The amount of energy each man's metabolic processes can supply seems not directly correlated with the power of his intellect. Shall we conclude that we all inherit also a certain quantum of a type of energy not derived from metabolism, and that our intellectual powers depend on this super-physical energy? Or shall we suppose that each of us inherits a central engine, the brain cortex, whose peculiarities of structure determine his intellectual ability by determining what proportion of the energy derived from metabolism can be used for thought? Not innate quantity of mental energy, nor the power of a super-physical engineer, but innate differences in the structure of the engine, seem to me the most probable basis for differences in the abilities of men.

Vassar College M. F. WASHBURN

EIGHT LETTERS FROM G. STANLEY HALL TO H. P. BOWDITCH WITH INTRODUCTION AND NOTES

In 1871, Bowditch and Hall, two outstanding American scholars whose names are often joined, had not met each other. One was a physiologist and at that time the other was a philosopher. In the year mentioned they both returned to the United States after having enjoyed prolonged periods of study in German universities. Bowditch was 31 years of age, he had proved himself a capable student in Professor Ludwig's famous laboratory of physiology at Leipzig; and he had just married Miss Selma Knauth of that city. He was returning bag and baggage (the latter containing well selected physiological apparatus) to accept a position as assistant professor of physiology at the Harvard Medical School and was soon to establish the "first physiological laboratory for the use of students in the United States."[2] Professor and Mrs. Bowditch ever maintained an especially close contact with German physiological science. Their home was a natural objective for many visitors (including Helmholtz) from abroad and many Americans going abroad had the way paved for them by the Bowditches. Hall, six years younger than Bowditch, had studied philosophical theology under Dorner in Berlin. He had become acquainted with Von Hartmann and other German philosophers and had done

[1]These letters were placed at our disposal by Miss Frances N. A. Whitman, Librarian of the Harvard Medical School.

[2]W. B. Cannon, Henry Pickering Bowditch, *Mem. Nat. Acad. Sci.*, 17, 1924, 183-196.

some work as war correspondent at the outbreak of the Franco-Prussian struggle. Returning to New York he became for a second time a student at Union Theological Seminary and a few months later received the B.D. degree. There is no evidence that Bowditch and Hall knew each other prior to about 1876.

Hall, as is well known, taught English, modern languages and several other subjects at Antioch College from the fall of 1872 to the spring of 1876, *i.e.* for four academic years. He tried to resign in the spring of 1875 as he wished to go again to Germany, this time to study with Wundt, whose *Grundzüge der physiologischen Psychologie* had appeared in 1874, but the Antioch College Board would not accept his resignation.[3] When Hall finally left Antioch in 1876 he apparently had definite plans for going immediately to Wundt at Leipzig.[4] From these he was deflected for a period of two years, during which he occupied a tutorship in English at Harvard. Of this period Wilson says,[5] "in spite of the large amount of required work [reading and correcting Sophomore papers], in which he took but the slightest interest, he found time to attend courses under Dr. H. P. Bowditch at the Medical School on Boylston street, and to work in his physiological laboratory on "The Muscular Perception of Space," which he presented as a thesis for the Doctorate in Philosophy in June, 1878.[6]

Just what circumstances brought Hall to the physiological laboratory we do not know. His reading of Wundt may easily have generated this interest. Bowditch was at this time the center of a very live group of workers. Certainly Hall and Bowditch had a strong common bond in their experiences and acquaintances in Germany. Just how much more it would have taken to swing Hall definitely and forever into physiology we can not judge. Apparently he was not far from that decision several times during the next few years. That his thinking and interests were greatly stimulated by his contact with Professor Bowditch is clear to see. Bowditch was carrying on the study of the growth of children before Hall left for Leipzig. Hall was later to refer to him as the "father of child study." Bowditch urged Hall to go direct to Ludwig, and Hall later wished that he had done so.

The four letters before us written by Hall from Germany give an interesting cross-section of what was going on fifty years ago in an important scientific period. Hall was keen for keeping in touch with every thing, he knew that Bowditch was keen to hear about everything; conditions were thus right for letters full of informing details.[7]

---

[3]Louis N. Wilson, *G. Stanley Hall*, 1914, 58.

[4]In Hall's *Life and Confessions*, he states (p. 204) that he did go to Germany in 1876, but the account by Wilson seems clear and trustworthy, also in the particular that Hall completed his thesis on "The muscular perception of space" with Bowditch in 1878 and got his Ph.D. from Harvard prior to this second trip to Germany, whereas from the *Confessions* (pp. 204-219) one gets the impression that the Ph.D came after the study with Wundt and Ludwig.

[5]Wilson, *op. cit.*, 64.

[6]G. Stanley Hall, The muscular perception of space, *Mind*, 3, 1878, 433-450. This publication bears little if any internal evidence of experimentation and there is no statement that it was prepared under the direction of Bowditch.

[7]We have transcribed the letters as accurately as possible, preserving Hall's spelling wherever it is clearly decipherable. Because of Hall's indifference to legibility as well as to orthographic exactness it has not always been possible to identify definitely certain proper names and a few other words.

21 Louisen Str. II, Berlin, 23/12 [1878]

Dear Professor Bowditch:

The winter recess has begun and I take great pleasure in writing you as you requested. I am indebted to you to an unexpected degree for the introductory card to Kronecker[8] to whom I already owe very much. He has taken pains to introduce me to most of the people here whom I care to know, into society and societies and given me a good start in my work. He has pleasant bachelor rooms in the new laboratory here and seems to be one of the most genial and popular men in the physiological society. He is quite grey, is fond of telling reminiscences of his acquaintance with you and as far as I can judge is practically at the head of physiology here. DuBois-Reymond himself has done but little scientific work since he took charge of the plans for the new institute some years ago. He still lectures however daily on general physiology, which with all the immense resources now at his disposal he makes so *anschaulich* that his lectures are little more than the explanation of curves, charts, instruments and experiments. He has just been reëlected president of the physical and physiological societies and celebrated his 60th birthday last week.[9]

I presume you have already seen an account of most of the interesting work done here.[10] The most important I think is that of Dr. Meyer with the new overflow thermometer. Glass balls filled with mercury are swallowed by many sorts of animals and in due time given again [*i.e.* recovered] *per anus*. As the temperature rises the mercury flows out through a capillary tube. They are then gradually warmed in water to the overflow point and the temperature measured by an ordinary thermometer. Some new results about internal heat are said to have been obtained. Dr. Gad[11] has invented a very sensitive lever for recording differences of airial pressure which he finds considerable between respiration through the nostrils, the mouth and through a small cannula (provided with a stop cock) and fastened into the trachea. Prof. Kronecker has varied Mosso's experiments on human limbs and on dog's feet before and after extirpation of the supposed vaso-motive center, etc. Christiani[12] is giving all his time to the study of spiritualism and *taschenspeletesrei* [sic] which he began by the study of a lady in Leipzic. Dr. Litten found that the kidneys of dogs enlarged after ligature of arteries and inferred a reflux of blood through the veins. This point has been debated in several meetings of the physiological society. A new worker whose name I do not know is getting manometric curves of a frogs heart under the influence of various poisons and to complete the list of workers, with *vivasectorium* [?] I am working with the spring kymograph and hope soon to send you the results in print.

I am taking three hours a day of Laboratory work; hearing Helmholtz on theoretical physics; Kirchoff[13] on mathematical electricity and Wangerin [?]

---

[8]Carl Hugo Kronecker (1839–1914) prior to this date had done his outstanding work on the fatigue and recovery of striped muscle and also had shown that the heart muscle cannot be tetanized. In this latter field of research he had the able coöperation of Bowditch.

[9]Du Bois-Reymond was born November 7, 1818 (d. 1896). The statement that he had just celebrated his 60th birthday dates Hall's letter as of 1878. It is interesting to note that this remarkable scientist like Hall was at first undecided as to the bent of his interests. He followed the great ecclesiastical historian Neander and occupied himself with geology; but finally he turned to the study of medicine with such zeal and ability as to attract the interest of Johannes Müller.

[10]It is not clear to what account he may have reference. We judge this is probably the first letter Hall wrote to Bowditch after his arrival in Germany.

[11]Johannes Gad, Die Regulierung der normalen Athmung, *Archiv. f. Anat. u. Physiol., Physiol. Abt.*, 1880, 1-30.

[12]Arthur Christiani.

[13]G. R. Kirchhoff, (1824–1887), physicist. From 1874 professor in Berlin; worked on the theory of electricity; 1860 with Bunsen discovered spectral analysis.

on differential and integral equations each four hours per week. Westphal's[14] clinical lectures on diseases of the brain and cord, Reymond general physiology, Munk[15] the senses and Harms [16] on psychology from three to five hours per week and a few free courses—the eyes, ear and brain once a week. So far I have been in good working order.

The clearest lecturer I ever heard is Kirchoff. He is so purely mathematical that he says all knowledge is definition and all science is at bottom only the description of motion. His course is more ordered [?] than any of Helmholtz. The latter is so often changing his symbols and even making and correcting mistakes that he is abominably hard to follow. He is planning more observations on retinal purple and it is said has devoted the best of his life to work of a more strictly psychological character of which his *Thatsachen in der Wahrnehmung* just issued is the beginning. When I think it out a little more I mean to have a talk with him about color perception.[17]

I must not forget to write that I said at the outset to Prof. Kronecker that he must draw no inferences about the work done in your laboratory from my proficiency, that I had only loafed about there and knew nothing about experimental physiology. He asked if Minot was a good representative of your workers. I said yes. He then pointed out in greater detail than in his note to *Die Genesis des Tetanus* wherein he thought Minot's work *Luckenhaft* [sic] etc.

The physiological society here which meets every two weeks numbers over 100 members and always adjourns to a Kneipe where all the men are exceedingly approachable and courteous. I have found several men who like myself are mainly interested in psychology. Munk is the other leading man in that society.[18] He has a splendid chance at the head of the veterinary school, and is spoken of as a most painstaking and reliable observer. Here he has thoroughly discredited Ferrier [?] where the observations of the two differ.

I came here because I found Wundt lecturing only in logic.[19] I shall probably stay through the next semester. I have just returned from the funeral services of Bayard Taylor.[20]

Professor Kronecker wishes to be very cordially remembered.

I should be very glad to hear from you and also to execute any sort of commission or errand in my power while I am here.

I remain very sincerely yours,

G. Stanley Hall

---

[14]Karl Westphal (1833–1890), neuro-psychiatrist, Professor at Berlin from 1869 and director of the Clinic of Nervous and Mental Diseases. Established the relation between lesions of the spinal cord and general paralysis and defined 'primary insanity.'

[15]Hermann Munk (1839–1912), physiologist. Professor at the Veterinary Hochschule Berlin. Worked especially on the physiology of the brain. It is interesting that in Europe noted experimenters in the field of physiology have been connected with veterinary institutes. Mollgaard at the Veterinary High School, Copenhagen, and Abderhalden at the Berlin Hochschule are other examples.

[16]Friedrich Harms (1819–1880), scientific realist. From 1867 professor at Berlin; follower of Fichte.

[17]Hall had already published Color perception, *Proc. Am. Acad. Arts & Sci.*, 5, 1878, 402-413.

[18]We suppose the comparison is with Hugo Kronecker.

[19]Wilson (*op. cit.*, 43) says "he went at once to Berlin and reëntered the University." This gives the reader a curious impression because one thinks of Hall as bound for Wundt. The letter clears up the point.

[20]Bayard Taylor died in Berlin, December 19, 1878, which gives us a second check on the date of this letter.

21 Louisen Str. II
Berlin, Sept. 3, [1879]

Dear Professor Bowditch:

I ordered the respiration apparatus as you wished at once. If you wish the isinglass plates will you please drop me a card. Herr Plath called today to say that as all he uses only those from America you might find it more convenient and cheaper to have them put in there. It cannot be done till Oct. Then I will have it sent *with them* unless I hear from you to the contrary.

I shall soon settle in Leipsic till Jan. perhaps till another summer where I mean to hear all Ludwig's lectures and do a little psycho-physic work which I have arranged with Wundt. I have begun to compile a sort of text book on psycho-physics or something of the sort. Cannot tell yet whether it will ever amount to much—especially as my liking for Professor Wundt's theories grows less and less but it will probably take all my spare time next year.[21]

I worked in Leipsic five weeks with Ludwig's assistant Von Kries[22] on a little theme of my own which I will send you soon printed though it is very unimportant.[23] The work with Helmholtz is not quite done as he had to order a new apparatus.

Should you start things this Fall so that I can get a chance to try my fortune at the Lowell Institute another year, as you say is possible, I need hardly say I shall be greatly obliged and will do my best not to discredit my backers.

I was so much pleased with Professor Ludwig and his Institute that I sincerely regret now that I did not follow your advice and go there at first last year. Though smaller, the Institut at Leipzic seems in every way more practical than the one here.

I am just here for a few weeks of loafing in Heringsdorf[24] on the East Sea, and am to be married in October to lady from Cincinnati.[25]

Very respectfully yours,
G. Stanley Hall

Leipzic, 15 f. Georgen Str.
Leipzic, Dec. 23, 1879

Dear Professor Bowditch:

I trust you have received, ere this, the box with respiration apparatus and the cannula sent by mail a few days later. The laboratory closed today for the hollidays and I write thinking you may be interested to hear even in an imperfect way what is being done there. Prof. Ludwig has been injecting peptone into the jugular of a dog and found new effects, in some cases resembling those

[21]We find no evidence of publications developing directly out of this interest in compiling a text book on psychophysics. Was Hall correctly rating his own interest in planning to undertake a text-book of psychophysics? Even living as next door neighbor to Fechner, often mentioned, could scarcely make of the versatile future "leader of American Psychology" (James) an exact experimentalist.

[22]Johannes von Kries (b. 1853, now Emeritus Professor of the Medical Faculty, Freiburg) measured the blood-pressure in the capillaries in 1875, and with Kuster published on color-blindness Ueber angeborene Farbenblindheit, *Archiv. f. Anat. u. Physiol.*, 1879, 513-524.

[23]Ueber die Abhängigkeit der Reactionszeiten vom Ort des Reizes (with J. von Kries), *Archiv. f. Anat. u. Physiol., Physiol. Abt.*, 1879, Suppl. Band, 1-10.

[24]Heringsdorf, scene of Hall's first station as war correspondent nine years earlier. Cf. Wilson, *op. cit.*, 40.

[25]An entertaining account of the incidents connected with this event are contained in Hall's Getting married in Germany, *Atlantic Monthly*, 47, 1881, 36-46. Hall married Miss Cornelia Fisher. The date is not quite clear, E. C. Sanford (Granville Stanley Hall, this JOURNAL, 35, 1924, 315) gives it as 1878 in Berlin, but Wilson (*op. cit.*, 45) says it was September 1879.

of curara, so peptone is all the rage just now. One worker is injecting it under different pressures, and with different electrical stimuli into the tongue of a frog and studying with a microscope its effects on the circulation. Another is extracting it from five or six distinct tracts of the intestines and measuring the quantity found after different diets and different stages of hunger. Another in the chemical laboratory is getting very different results from Meissner's[26] as to its nature. Next in importance, I think, is a work on uric acid by a Russian here. It is a long work. He exterpates the kidneys of several sorts of fowls and animals and kills them in a few hours, cuts them up and attempts to determine how it occurs in muscles, intestines, etc. Von Kries, besides a new *copper* contact, which he argues is better than quicksilver, has found a stage in the electric stimulus of muscles, about the beginning of the maximal where there is a strange lessening of the amount of contraction, which when that point of intensity is passed, becomes the actual maximum.

Another is working on the effects of two stimuli applied directly to a muscle (without a nerve) when they meet. Another is working on the phenomena of erectile tissues under nervous stimulation. He now thinks that instead of the common hypothesis the first stage of erection is the expansion of the *arteries*. I have been at work a month on reflex. I have succeeded in irratating about five mm. of a frogs spinal cord containing the reflex organs of its hind legs intact and with circulation through it normal and *think* I have got a law for *erregungszeit* as a function of strength of irritation. It promises to be rather a long work, and is my first alone. My chief hope is in Ludwig's new machine for giving irritations of any constant rate and at the same time shutting off either opening or closing stream at will.

Flechsig[27] is following up his "development method" now on cells. He has a new method of preparing foetal brains for the microscope. I have not yet learned what it is and think he does not tell, though he often refers to it. He seems to have now quite a psychologic drift. I forgot to mention young Voit, who is doing some great work on the gas of dyspnoeic blood.

Of course this is all *very* inadequate as almost none of the men have made anything public. There is much in the chemical wing that I know nothing about.

There has been great excitement about vivisection. Ludwig receives threatening letters and one of his assistants has been sued for aspersing the motives of von Weber, the chief agitator. There was great danger at one time that public opinion would be too strong for the physiologists, but I think the character of the agitation and the respectability of the prejudices against it is now slowly and surely declining. The students gave Ludwig quite an ovation the other night after an abusive lecture of v. Weber's.

Wundt, with whom I am still working four evenings per week, is greatly excited about Zöllner's[28] last book, which reflects upon him. Prof. Westphal told me last year that he considered Zöllner, unquestionably insane. Yet Wundt tells me spiritualism is spreading rapidly among the wealthy "half-cultured" classes all over Germany. Socialism is the third excitement here. Forbidden papers are thrust into your hands by students, and an active propanganda seems to be kept up. Bebel,[29] the chief socialist here, a member of the

[26]Georg Meissner (1829–1905), physiologist. From 1860 professor in Göttingen.

[27]Paul Flechsig (b. 1847, now Emeritus Professor of the Medical Faculty, Leipzig), psychiatrist. Developed the genetic method for the study of the brain and spinal cord.

[28]J. K. F. Zöllner (1834–1882), astronomer and physicist. From 1866 professor of physical astronomy Leipzig. Hall's statement probably refers to one of the four volumes of the *Wissenschaftliche Abhandlungen* published 1878-81. These contained a critical philosophical review of the bases of contemporary natural science and a defense of Weber's Law. Considerable space was given to spiritism and to hypnotism and to presentation of a four-dimensional space.

[29]August Bebel (1840–1913), the well known socialist, recognized leader of the party from 1890.

Reichstag and whom I met one evening, seems a simple earnest man; but I think one sees his work and that of his colleageues in even the washwomen and servants here.

I have lately tried to find out by reading the original in the Arbeiten here, whether Wundt correctly interpretes your work (p. 122 etc. part II, Mechanik des Nerven) I cannot think it favors the vague and at least entirely *un-anatomical* theory which he developed before and after, and with which he tries to bring it into harmony.

I heard today that the Berlin physiologists think Herman's[30] new physiology a *tendency* work and directed mildly yet decidedly against them.

I trust this letter is not *quite* unpardonably long. I write in a bit of haste as I am off to So. Germany this P. M. I remain

Very sincerely yours,
G. S. Hall

Physiology Lab. Leipzic
Feb. 24, 1880

Dear Professor Bowditch:

Yours was received two or three days ago. Dr. Gaule[31] tells me that the peptone, concerning which you inquire—for frog heart solution—need not be pure. He uses Witte's of Rostock and thinks many of the preparations in the market might do as well. He now uses 0.6 per cent salt solution and 0.004 per cent NaHO—and refers to his Die Leistungen des entbluteten Frosch hertz[ens] in DuBois, Archiv. 1879 [1878], which of course you know and to Dr. Schmidt Mülheim's peptone study which Dr. v. Kries sent you two days ago.

The work in Pflugers XIX which you think has made Voit's results questionable, Ludwig speaks of just as you do. The question he says is open and further experiments must decide which view is right.

I forgot to say that I learn that since Gaule and Schmidt Mülheim's[32] work, a man here by the name of Bunsen has still more striking results, which will very soon appear.

I find I made a great blunder in giving the first rumors I had heard respecting v. Kries copper contact. It is simply a copper instead of a platinum wire point to plunge into the quicksilver and his work was in the line of and in fact seems to be suggested by Donder's[33] study of the oscillation of the opening induction stream (Proces. Verhand. von der Academie zu Amsterdam 1868, Vol. 1)

The reason why v. Kries and I found both an Unterbrechungskreis *and* a Reizungskreis necessary[34] was only because the magnet did not let go the vibrating rod promptly and regularly enough. So the second Kreis was to give the spark when the rod actually began to vibrate. A trifling error but one which I think from results with Wundt's *Hippsche Kronoscope* sufficient to vitiate our results with that instrument.

I am much obliged for your opinion about the extent of the heart's contraction not being caused by peculiarities in its muscular substance apropos Wundt. My work with Helmholtz concerning which you inquire it seems possible will not be completed. He wrote me in October that the instruments

---

[30]Ludimar Hermann (1838–1914). From 1868–1884 professor of physiology in Zürich. Probably Hall refers to the *Handbuch der Physiologie*, which appeared in six volumes from 1879–83.

[31]Justus Gaule.

[32]Adolf Schmidt-Mülheim, Untersuchungen über die Verdauung der Eiweisskörper, *Archiv. f. Anat. u. Physiol., Physiol. Abt.*, 1879, 39-58; also *ibid*, 1880, p. 33.

[33]F. C. Donders (1818–1869), professor in the University of Utrecht.

[34]See the article mentioned in our note 23 above.

he had promised would not be ready for some time and so now I am deep in my reflex work here which I think on the whole more promising. I shall spend the spring vacation writing up a history of reflex A at Ludwig's suggestion.[35] I don't know how my work pleases him but it certainly interests me far more than my work with Kronecker did.

Ludwig is immensely annoyed by the anti-vivisectors,[36] Weber has prosecuted one of his assistants—Gaule—for libel and L. thinks it very fortunate now that physiology did not go into the *philosophical* faculty as he was at one time inclined to wish, for if it had, the medical men would not have stood by him so effectively in this vivisection war as they have done. Ludwig is making a pure empiricist of me and if I had worked with him when I was here eight years ago instead of my philosophy, I might hope to become a decent experimenter. Any how that is all my aim now, late in the day as it is.

Very truly yours,
G. S. Hall

The above four letters have shown us what Hall was thinking and doing in 1878-1880. They read like a vividly annotated table of contents to Du Bois-Raymond's Archiv for that period. On returning to the United States in 1880 Hall and his wife took up residence in the vicinity of Boston. Apparently he now found time for doing some further research with Bowditch, although the joint paper which they later published[37] does not state just when their interesting experiments on illusions of motion were performed.

The four remaining letters which we have the pleasure of presenting are all rather short. Two were written at Johns Hopkins and two of them at Clark. They give interesting and unstudied side-lights on Hall and some of his many scholarly interests and projects.

Johns Hopkins University
Baltimore Feb. 14, 1885

Dear Professor Bowditch:

Great was my surprise and pleasure to see that you had not only interested yourself in psychic research but undertaken to investigate the most sublimated department of it, viz. *Thought transference.*[38] I now received [?] a program of a discourse by you on the "Nature of Nerve-force"[39] I am so profoundly skeptical about thought transference, and so grossly [?] ignorant about the nature of any kind of force that I feel very sure you must have access to new sources of knowledge sealed to me of which I am thirsting to know.

Seriously however, I wish to ask you if you are experimenting on thought-transference and if with results. We (*i.e.* I with a few pupils) are trying it quietly at my house on a few excellent hypnotic and one cataleptic subject,

---

[35]See Hall, A sketch of the history of reflex action, this JOURNAL, 3, 1890, 71-86. This article gives no note or statement indicative that parts of it had been prepared ten years earlier in Germany. C. F. Hodge (A sketch of the history of reflex action: I. Beginnings and development to the time of Charles Bell, this JOURNAL, 3, 1890, 149-167), refers specifically to the work done by Dr. Hall on the subject "about the year 1880." Hodge made use of Hall's notes.

[36]See Hall, *Life and Confessions*, 205, bottom paragraph.

[37]H. P. Bowditch and G. Stanley Hall, Optical illusions of motion, *J. Physiol.*, 3, 1882, 297-307. This paper seems to give internal evidence that at least some of their experiments were done after 1880.

[38]Bowditch aided in founding and for several years in managing the American branch of the Society for Psychical Research.

[39]H. P. Bowditch, Note on the nature of nerve force, *J. Physiol.*, 6, 1885, 133-135.

with valuable results on the nature of the precautions to be observed, and the subtlety of sensation in hysteroid subjects, but with no trace or shadow thus far of any thought-transference in the sense of the English Society.

I am mortified about your books which I have had so long, and if you are not using these I'd like them a *few weeks* longer as the work is now actively going on which we began.

Very sincerely yours,
G. Stanley Hall

I wish I could know what you think of the whole business, I feel about like withdrawing from the society, but hope you will not, for I sh'd certainly follow you.

Johns Hopkins University
Baltimore, April 19, 1885

Dear Dr. Bowditch:

My tastes, interest, reading etc. draw me more and more to make psychiatry, scientifically and not practically worked, the focus of my psychology, in which I did some work abroad including the courses of Westphal, Wermke and Meynert[40] and Flechsig, with whose writings I am acquainted with detail. If I had a degree of M.D. it would open here (and a summer or two I mean to spend abroad soon)[41] opportunities now closed but which I think I could utilize. For these reasons I venture to ask you, not as Dean but only as a friend for information and advice.

On the one hand, I think the practice of honorary degrees, particularly that of M.D. entirely and wholly bad. It is opposed to all my ideas of higher education. Yet on the other hand, the practice still exists and why should I not avail myself of its advantages for study. I should never practice of course, but use it only to procure entree to opportunities in that one line which seem to me the key of all psychological studies as well as educational ones.

Will you at your leisure have the goodness—for I know no one else to whom I can turn—to advise and instruct me.

Ever sincerely yours,
G. Stanley Hall

Clark University
Worcester, Mass.,
May 25th, 1889

My dear Dr. Bowditch:

Enclosed please find your V.[olume] which I return with thanks.

I wish you would let my Journal print some, or all, of those splendid things you are about to publish.[42]

The fact is it is very hard to get good original articles for the Journal, moreover, it does not pay expenses and I myself pay every cent of the deficit.

I feel that the fine print notices, which cost us a *great deal* of labor and which are made as complete as possible by *buying everything whatever* that seems likely to be good in that line and which is a private expense of considerable magnitude in addition to the deficit, are really an evidence of, at least an intention to render a real service. This makes me at any rate freer to ask men like you for articles like yours.

---

[40]Th. H. Meynert (1833–1892), from 1870 professor of psychiatry and director of that division in the General Hospital in Vienna. See Hall, *Life and Confessions*, 215.

[41]Parenthesis added by the editors.

[42]Bowditch never published in the American Journal of Psychology, and only two of his articles are there reviewed (see I, 1888, 712 and 3, 1891, 577). For the bibliography of Bowditch see Cannon, *op. cit.*, 195-196.

Our rule is to pay for no reprints or plates, from the first number no exceptions have been made to this rule, but we are ready to make an exception at least for reprints and *possibly*, *possibly* for plates in your case.

Hoping very much I may be able to see you before you leave the country, I am,

Ever most truly yours,
G. Stanley Hall

Clark University
Worcester, Mass.,
April 22, 1891

My dear Dr. Bowditch:

Dr. Boas has been subjected to violent attacks from a local paper here for wanting to measure school children. The paper has made many blind references and statements about your work, such as, that you sold the results of your measurements to hatters, clothiers, etc., that you have said they were of no use, were a failure, etc. etc. Of course we cannot reply nor pay any attention to anything on so low a plane, and you probably would not wish to do so, at the same time I should be very glad to have in my possession for future contingencies, with power to use or not at my discretion, a line or two from you covering these two points—whether you made money, and whether you ever said that your measurements were of no value.

If you felt like adding a line indicating what value they were for either Anthropology, Pedagogy, or anything else I should be very glad.

I hope to send you before long a few brief notes on the extremely interesting drawings.

I am, Ever Yours,
G. Stanley Hall

P.S. It is a ridiculous mess but with a few words from you as a reserve I may inspire courage. G.H.

The period covered by the last four letters, 1885-1891, was crammed full of professional work for both correspondents. It is possible in this work to trace a certain parallel of activity and interest. Bowditch was active in the founding of the American Physiological Society in 1887 and became its second president the following year. Similarly, Hall was prominent in the founding of the American Psychological Association accomplished in 1892 when he became its first president. From the establishment of the British Journal of Physiology in 1878, Bowditch was active as an American editor and contributor to this important channel for physiological literature.[43] Hall was of course a much more active editor. He established the American Journal of Psychology (1889) and as its first editor contributed both articles and reviews in large number. Bowditch continued to carry on laboratory studies and through this channel contributed to physiology, anthropology and pedagogical methods. Hall's work was not so clearly of a laboratory nature but was none the less fruitful and stimulating and covered a much wider range. Both men were inevitably crowded into administrative work, although they tenaciously held on to teaching and were both eminently successful in this capacity.

In the letters before us the younger man turns for guidance to his senior, almost as to his older self. Hall's intellectual inheritance was doubtless chiefly Euro-

[43]Bowditch continued in this capacity with the Journal of Physiology until its 27th volume published in 1902. He was also one of a small group of Americans to inititate the American Journal of Physiology, January 1898.

pean but it is obvious from the present intimate glimpse that much had come to him through contact with a leader of American Physiology, whose words he apparently more than once counted on "to inspire courage" as well as to "advise and instruct."

Stanford University

W. R. MILES
C. C. MILES

A PROTEST

A review of Porteus and Babcock's *Temperament and Race* which appeared above the initials "J.P." in a recent issue of the JOURNAL (Vol. 40, 1928, 640 f.) contains so many misapprehensions and misstatements on the part of the reviewer that I feel it calls for reply. In some respects the methods used and the conclusions reached by the authors are absolutely at variance with those attributed to them by the reviewer. The assertions that need correction are taken hereunder in serial order from the review.

(1) "Intelligence tests are included as measurements of temperament." This can only apply to the Maze tests which insofar as they measure a tendency to use foresight and prudence in the working of the tests, are tests of temperament.

(2) "A number of photographs of 'typical' boys and girls of the several 'races' are included. How 'typical' they are seems wholly a matter of personal judgment." On the contrary the children were selected because their measurements approximated most closely the average measurements of the whole racial group. This is indicated on the photographs as follows: "Typical 9, 12 14 year Japanese (or Chinese) boys approximating the average in racial head form." Hence 'personal judgment' did not enter into the selection at all. Further, the two photographs of 'typical' children in the book were boys and not 'boys and girls.'

(3) "Twenty-five judges (not described) are asked to estimate Japanese . . . as races according to planning capacity [and other traits]." If the reviewer had read the book carefully he would have found at the first mention of the judges a description of the group as follows: "Ratings were obtained from twenty-five people of long experience in the islands, observers whose work brought them into very intimate contact with the various racial groups. Sixteen of the judges were plantation managers while among the remainder of the group were head workers of social settlements, plantation doctors, and several educationists" (p. 90).

(4) "One is forced to ask how independently the judges acted of popular prejudices and of the bias of the authors." With regard to popular prejudices, it is, of course, impossible to say how much of these entered in, but the authors took pains to point out that in rating the Filipinos, Portuguese and Chinese in general, and the Japanese in aggressiveness, some prejudice might be expected. As regards freedom of the judgments from bias of the authors, we certainly did not think it necessary to state that the judgments were absolutely independently given, but took it for granted that we should be credited with ordinary scientific honesty.

(5) "Absolute brain capacity is measured by an X-ray photography method worked out by Stevenson and Porteus, the validity of which seems not to have been established." The brain capacity was *not* measured by photographs. As stated on page 169, it was done by taking the actual head measurements of about 4000 children. It is true that in a discussion of methods on page 139 it was incidentally remarked that if the method worked out by Stevenson and Porteus is used, error would be reduced. The reviewer is under a quite unnecessary misapprehension as to the method employed.

(6) "Between such capacity of the head and intelligence and certain desirable temperamental traits, on the other hand, a high correlation is found." A high correlation was not found. On the contrary, between intelligence and brain capacity the correlation is stated to be "probably about the order 0.3." The authors distinctly state that this correlation is not high (p. 150).

(7) "There is a suspicious enthusiasm for the method." Again and again the authors point out the pitfalls of the method and its unreliability as an individual diagnostic measure, *e.g.* "it fails to provide a reliable key to individual intelligence," (p. 138); "it is a difficult problem to state the connection of brain volume with intelligence" (p. 140). (See also pp. 147, 148, 173.)

(8) "The number of cases is, however, small and there is little satisfactory evidence that the samplings are good." The number of Japanese measured was 1307 and Chinese 1027—quite respectable totals. Further, all the evidence possible that the samplings are good was presented. "All of the children attending three large schools were measured and all the boys in a fourth" (p. 169).

(9) "We read that brain growth continues through the whole period of growth in stature even after all the 'powers' both mental and physical, have reached maturity; moreover, that whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists. This is all to be taken merely on the assertion; no evidence is weighed or presented." With regard to the evidence for the continuance of brain growth through the period of growth in stature, the reviewer's criticism is entirely mistaken. On page 153 a graph shows the brain growth curves for 1547 normals and 295 male defectives from 13 to 21 years, followed by a table on the next page. With regard to the second statement, the authors really did not consider that such a well known fact required evidence in its support. We thought that everyone was familiar with the microcephalic idiot or imbecile in whom brain growth ceased or was arrested at an early age.

(10) "Brain size is also in close relation to cultural level in the different races." Our statement reads, "A comparison of living races also shows *a* relation between average brain size and cultural level" (p. 143). Surely the reviewer sees a difference between ' a close relation' and 'a relation.'

(11) "Boys are found to be superior to girls both in brain capacity and in intellectual ability and initiative. There is a blissful disregard of most careful work done by psychologists on sex differences." This statement also requires refutation in parts. Boys were *not* found to be superior to girls in intellectual ability. Learning capacity is held to be equal if not superior in girls. We say on page 160, "Returning to the subject of sex differences, we may observe that all the evidence that has been collected with regard to sex differences appears to prove that the differentiation in male and female post-pubescent brain

growth is not related to differences in learning capacity. There is almost general agreement that females are equal if not superior to males in this respect." The authors then proceed to quote Burt and Moore with regard to female superiority or equality in memory tests and tests of higher reasoning processes. We did not make this statement attributed to us and hence we certainly would not cite authorities in its support.

(12) "For tests of intelligence, as well as of temperament, the authors . . . prefer the Porteus Maze." Here again the reviewer is mistaken. The author of the Maze test has never, in this publication or any other, preferred the test as a test of intelligence. Conversely, he has by correlation and otherwise shown that it has only a "fairly close" relation to tests of general intelligence, the correlations being "usually of the order 0.5 or 0.6" (p. 289). There is not one word in the book which is intended to indicate such a preference.

(13) The reviewer states that an historical sketch of race studies is "very inappropriately selected as to representativeness" but neglects to say that later on a whole chapter is devoted to previous studies in which the investigations by such workers as Brown, Arlitt, Garth, Kimball Young, Brigham, Colvin and Allen, Murdock, Pintner, Davenport and Craytor, Darsie, Young, Dickson and Fukuda are discussed.

(14) With regard to one criticism we agree that "J.P." is correct and that is where he states that the annotated bibliography is "remarkable for important omissions." It is unfortunately true that we neglected to list one very important work, "A Study of the Differences between White and Negro Children" by Joseph Peterson. Our manuscript left our hand in 1924 and this study, though bearing the date 1923, did not reach us until a later period and hence was not available for listing and annotation. For this omission of Joseph Peterson from the list we would crave J. P.'s indulgence.

In concluding, the reviewer passes some very severe strictures on the book especially with regard to what he calls the "marked freedom on the part of the authors from the usual limitations upon scholars of careful conformity to established facts." If the book really contained what the reviewer says it does, this criticism might be considered just, but as the case actually stands this condemnation is much more applicable to the review than the book. The latter is by no means exhaustive or complete in its treatment of the difficult question of racial psychology and many would agree that much of its data is suggestive rather than conclusive. But surely it is extremely unfair to attempt to make the authors responsible for the reviewer's misapprehensions which can only be due to a hasty or careless reading of the book.

University of Hawaii S. D. Porteus

A Reply to Porteus' 'Protest'

There was no intention of concealing who 'J. P.' is; my name occurs at the beginning of the review section, and in accordance with well established editorial practice, there is no need of repetition. The identity of the reviewer appears clear even to Mr. Porteus. I shall reply briefly to each of his "corrections" in order; but even if I were wrong in all the minor points indicated, that would not prove that my general evaluation is at fault.

(1) On page 289 the authors say: "The relation of maze test performance to so-called 'general intelligence' is fairly close." Correlations with the different intelligence tests are not very high (usually 0.5 or 0.6), we are told, but the whole treatment rightly indicates that these test scores do not register all the aspects of intelligence. There are many statements which imply that the maze test measures several important aspects of intelligence, *e.g.* "In general, therefore, the maze test provides a measure of the individual's ability to adapt his methods to the solution of a problem, the most intelligent attitude to which is one marked by forethought and prudent working" (p. 285). From there on, to page 300, this general position is taken. How shall one differentiate 'intelligence' from such mental functions? In Tables 29, 30, 31 (if I read correctly) maze scores are converted into IQs.

(2) There are numerous photgraphs in the book besides those of the boys who are listed as *typical*. These photographs involve both boys and girls, as well as adults. What are they there for, if not to typify or represent in some degree their respective races? (See pp. 32, 38, 46, 54, 64, 100). Are they not there to catch the public eye?

(3) This hardly needs a reply. But note: "among the remainder of the group" were so and so. Is this a good description of as important a factor in this experiment as were these judges? The reader of the book should note the descriptions of the several races by the authors, and then compare these descriptions for closeness of agreement with the *later* results by the judges. Does careful scientific work not require that causes of the close relationships *thus established* should be analyzed? The authors selected the judges. It is clear that several common influences operated toward the final high agreement. Were the descriptions taken from the later results of the judgments, or were they not? The reader is entitled to know.

(4) This is answered in (3).

(5) The reviewer did somewhat misunderstand the situation here, but there were some grounds for this misconception. Readers may, however, expect something more definite than general statements as to the reliability of such "brain capacity" measurements as were made, and also as to *how much* more accurate this X-ray method is than the method which was used. We should also have some quantitative comparisons with brain volume otherwise measured in post-mortem examinations.

(6) The "high correlation" to which I referred is not that given as 0.3 by the authors. My idea was gathered generally from statements like these, and an argument running through more than fourteen pages: "The study of the few human remains that have been preserved to us from historic times *would also lead to the conclusion* [italics mine] that along with the rise in culture there has been a corresponding evolution of the brain" (p. 141); "Another race which shows educational aptitudes proportionate to their average gross brain development is the African negro" (p. 145); "It is our belief that this maturing of the power of learning capacity does not by any means signify the ripening of all the mental powers and that the brain growth occurring after this age [14 or 15 years] is significant and important as subserving the development of mental traits which the psychometric yardstick is too short to measure" (p. 154). I can supply pages of quotations from the book represent-

ing the same view. In this chapter the authors admit that various pathological conditions associated with low mental level are accompanied by above-normal development of head size, thus lowering the correlation of mental level with head size, and they also explain, quite rightly again, that Pearson's low correlations between mentality and head measurement cannot be taken to indicate a low correlation with brain volume, because only single head measurements were used. All this is in line with the general position as to a high relationship, taken by the authors in this chapter on Brain Capacity and Intelligence. (The quotation attributed to me under 6 is not exactly my statement.)

(7) As to this enthusiasm the reader must judge. Pointing out a few limitations does not mean the absence of an enthusiasm. Indeed, on the strength of the authors' apparent satisfaction with the Porteus Maze test I am having it and certain other tests applied to a number of negroes, to determine how well this apparent satisfaction is justified. I, of course, agree with the authors that existing intelligence tests are unsatisfactory.

(8) The number of cases as given on pages 160 and 170 for any year and sex ranges between 24 and 78 for the Chinese children and between 27 and 100 for the Japanese. These numbers are not large. It is not a question of *totals* for both sexes and all the ages from 6 to 15 years, and that is just where the book is misleading to the general reader. The big total numbers seem even to have misled the senior author!

(9) Again numbers are not large in some of the age-groups compared in the curves on page 153. In the defectives they run from 41 to 73. Surely the authors will not try to defend the statement, that "whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists." This is only another general statement by them to support the close relationship between intelligence and brain volume or "capacity," to which I said the book holds. See (6) above.

(10) This criticism is only a quibbling over the points discussed under (6), and needs no further consideration here.

(11) Porteus here diverts the issue to matters of "learning capacity," "memory," etc. The authors' general position is that rather marked sex differences in certain important intellectual powers are well established, if I understand them. There is a confusing use of terms like 'learning capacity,' 'initiative,' 'ambition,' etc. But note this: "It is possible that we may ascribe this lesser female brain capacity in other years [than 12] to the lessened general bodily size of the girl. But unfortunately for the advocates of sex equality, from 14 years onward the boys steadily increase their superiority so that from an average of 62 cc. at 14 years of age it is increased to 128 cc. at maturity. This advantage in males is due not to any remarkable spurt in their development but simply to a retardation of brain growth in the female" (p. 158). This "lack of initiative [in women] is explained by female apologists as being entirely due to an attitude that is enforced on women by convention and public opinion" (p. 161). I here re-assert my charge of "a blissful disregard of most careful work done by psychologists on sex differences." Much more evidence for my position can be supplied if necessary.

(13) and (14) The authors give first a very sketchy and, I must repeat, "somewhat inaccurate" account of race studies. Woodworth's race tests

for example, are placed in the Chicago Exposition. In later chapters they discuss several other such tests. Since this later account also has defects of the kind mentioned in my characterization of the first one, it was not considered necessary to discuss it separately. These two accounts, and also the annotated bibliography, leave out several studies—besides my own monograph!—which were published early enough to have been considered. The authors rightly point out the inadequacies of the army testing, and yet on the basis of these army tests they say (referring to the twelve-year-old level of mentality arbitrarily set in early Binet testing as the borderline of feeblemindedness in adults) that the army examinations "proved that there were many millions of people below the Binet age who are getting along perfectly well in the community" (p. 187). It is, of course, a question of opinion whether the authors' accounts of the race testing is adequate. I accept many of their statements, but cannot agree with Porteus' apparent estimate of the general sketch.

It would be possible to go ahead and point out numerous inaccuracies and unjustifiable categorical statements in the book under consideration, in justification of my evaluation of it, but I shall not indulge in this sort of attack. If Mr. Porteus could see the marginal notes in my copy he would not charge me with hasty and careless reading of his book! Let it not be inferred from these replies to his 'corrections' that I see no merits in the work of these authors. They have done an extensive piece of pioneering work, which is sure to stimulate further research. It is only by continued investigations that we reach in time something that is thoroughly satisfactory and reliable.

George Peabody College JOSEPH PETERSON

THE THIRTY-SEVENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-seventh annual meeting of the American Psychological Association was held at Columbia University December 17–29, 1928. A registration of 537 members and associates makes this the largest meeting in the history of the Association.

At the business meeting 16 new members and 206 new associates were elected. The election committee reported the election of Dr. K. S. Lashley, of the Chicago Institute for Juvenile Research, as President for the year 1929. It was decided to merge the 1929 meeting of the Association with the meeting of the Ninth International Congress which will be held at Yale University September 1 to 7. Provision will be made for the address of the President as a part of the program of the Congress. The Council of Directors of the Association was given power to deal with all matters of business usually handled at the annual business meeting of the Association. The rapidly increasing size of the association has made the annual banquet such a serious problem that with the unanimous support of the members it was voted that the banquet be discontinued hereafter. This interesting social feature of the meetings will doubtless be replaced by dinners and smokers for smaller groups.

Compared with earlier meetings of the Association there were relatively fewer formal papers and more round tables for discussion of special problems. Certain interesting shifts in emphasis were perceptible in the program as a

whole. For instance, there were two sessions devoted to animal psychology at which ten papers were read, while for mental tests there was one session with five papers. One session was devoted to the Psychology of the Emotions, and of the five papers read three dealt with the galvanic reflex and a fourth was concerned with other bodily expressions of the emotions. There were only four papers read at the session for Educational Psychology and at least one of these might have been included within the Experimental group. There were no sessions devoted to Applied Psychology or Industrial Psychology, and no papers included in other sessions that would seem to come under this head. As the program was necessarily constructed out of the material offered by the members of the Association, the papers presented give an interesting index of the trends of research. One session was devoted to General Psychology with five papers, two to Experimental with twelve papers, one to Child Psychology with five papers and one to Abnormal with five papers.

Two sessions were arranged for nineteen Informal Reports by Graduate Students. These reports were of uniformly high quality and comprised a valuable part of the program. They were drawn from twelve different Universities, and covered a very wide range of research interests.

Six Round Table discussions were scheduled, each with a leader and a designated group of persons ranging in number from 25 to 60 who had indicated an interest in the topic. The topics assigned were: Clinical Psychology, Personality, The First Course in Psychology, Consciousness and Behavior, Psychophysical Measurement, and Esthetics. These meetings were, on the whole, disappointing. The groups, more than a hundred in every case, were too large to preserve the atmosphere of a round table, and the contributions in many instances were not constructive. For example, in the discussion of Consciousness and Behavior there was little or no progress beyond the search for suitable definitions of such terms as Behavior, Adjustment, Situation, Consciousness, etc.; and in the case of the First Course in Psychology the contributions were limited mainly to an account of how different individuals teach the first course, with no progress in the direction of a 'one best way,' if such there can ever be. The experiences of a seminar group, which worked under the direction of R. M. Elliott and spent six weeks upon an intensive study of the First Course problem (a resumé of which was presented at the Round Table) suggests that Round Tables will have to give way to Work Tables, consisting of a number of small, carefully chosen groups to study very specific aspects of a problem.

One of the most profitable discussions occurred in the Round Table on Psychophysics where attention was directed to such specific problems as the use to be made of 'equal' judgments in the psychophysical methods and the relative significance of the threshold and of some measure of variability such as the standard deviation of the judgments.

The conference of experimental psychologists, under the chairmanship of R. S. Woodworth, held two sessions. The meetings were informal. After a census of the topics upon which members were prepared to report, sensation was chosen as the first topic. In this field experimental studies of audition were most prominent. Among later topics the nature of the learning curve received some attention. From the point of view of the mathematics of the

curve and from experimental data, it was suggested that the present form of the learning curve is unsatisfactory.

The annual banquet held at the Hotel Pennsylvania was attended by 238 persons. About 150 others came to hear the address of President E. G. Boring which followed the dinner. James McK. Cattell, who reported concerning the Ninth International Congress, pronounced the group the largest gathering of psychologists in history. Dr. Boring's address was entitled "The Psychology of Controversy." By citing instances of controversy among great psychologists past and present he showed that controversy, even of the highly personal sort, may be an instrument of progress, and is an almost necessary accompaniment of the clash of brilliant minds and keen interests in research.

Columbia University A. T. POFFENBERGER

UNLEARNED BEHAVIOR OF THE ALBINO RAT

The following observation of an unlearned reaction in the albino rat was made several summers ago. The author was visiting a commercial rat colony in Philadelphia. Rats were sold with ages known to within three days. The adult females had their litters in individual cages and kept them there until weaned. Then all the young rats born within the three day period were placed together in a large cage. These cages had wooden sides about 2 ft. high, a wooden floor about 6 ft. by 3, and a half-inch mesh wire screen over the top. The visit was made on a hot, humid day. From 250 to 400 rats were kept in such a cage.

The author was surprised to find a great many rats hanging from the wire mesh apparently dead. The first interpretation was that the rats had jumped to the screen for air or to escape and had caught their noses in the wire. Actually, these rats were sleeping and maintained the position by hanging by their upper teeth from a wire of the screen. If awakened by scratching the nose, they would throw back the head disengage the teeth from the wire and drop into the milling mass below. So far as I can discover, no mention of this form of behavior occurs in the literature. The observation was reported to Professor Henry H. Donaldson of the Wistar Institute of Anatomy and to a number of other individuals who have had a wide experience with rats, but no further information was obtainable.

This reaction was probably unlearned, although there is the remote possibility that each group of rats worked out an identical reaction as the appropriate one to this new set of conditions. It will be remembered that the rats in a given cage were all born within a three day period; that they were never placed with older rats (except the mother) and were then in smaller cages; and that they were observed sleeping in this hanging position from all of the large cages. The reaction is obviously purposeful: it brings the rats out of the press on the floor during sleep and allows the cooler air to get to all sides of the body.

This observation is reported primarily to get it into the records but also as a beautiful example of the fact that one may have the appropriate neural mechanism for unlearned behavior (the author almost used the word instinct) totally unsuspected until the appropriate stimulus comes along to set off the reaction. The type of cage which has been developed for handling rats in the experimental laboratories, with the wire on the front, would not present the conditions for proper stimulation; lack of crowding removes another condition.

The author suspects the essential conditions are: (1) a cage of such form that the reaction is physically possible; (2) overcrowding; and (3) heat and possibly humidity. He has never had the chance to make observations on another group of rats to analyze the conditions. This note is published in the hope that further observations may be made.

University of Pennsylvania SAMUEL W. FERNBERGER

A BIBLIOGRAPHY OF THE ATTEMPTS TO IDENTIFY THE FUNCTIONAL END-ORGANS OF COLD AND WARMTH

During the years since the discovery of the temperature spots[1] a number of investigators have attempted to identify the functional end-organs. Two methods have in general been used; a direct method in which the tissues under a cold or warm spot have been excised and examined histologically and an indirect method in which correlations have been drawn between the spread and distribution of the cutaneous end-organs and the prominence or absence of the specific cutaneous qualities. The direct experimental method, the first employed, has been used five times. It was used by Donaldson in 1885,[2] by Goldscheider in 1886,[3] by Haggqvist in 1913,[4] by Dallenbach in 1917,[5] and by Pendleton in 1928.[6] It has thus far led to negative results; nerves and undifferentiated endings but no specialized end-organs have been discovered in the excised tissues. The indirect method, used first by von Frey[7] and recently by Strughold,[8] and Strughold and Karbe, has led to positive correlations which though tentatively advanced, have been dogmatically accepted.

The purpose of this note is to call attention to Goldscheider's study[3] by the direct method. For some inexplicable reason this has been overlooked; it is not mentioned in any of the investigations which follow it. It is an excellent study, done with the thoroughness that characterizes all Goldscheider's work, and deserving of a prominent position in the literature on this problem.

K. M. D.

---

[1] Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Upsala läkareförен. färhandl.*, 18, 1883, 87 ff., also in *Zsch. f. Biol.*, 20, 1884, 141-156. Alfred Goldscheider, Die specifische Energie der Temperaturnerven, *Monatshft. f. prac. Derm.*, 3, 1884, (cf. *Ges. Abh.*, 1898 Bd. I, 53-76). H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 399-416.

[2] Donaldson, *op. cit.*, 408 f.

[3] Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerves beim Menschen, *Arch. f. Anat. u. Physiol., Physiol. Abtheil., Suppl.-Bd.*, 1886, 191-227.

[4] Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 45, 1913, 57 ff.

[5] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 424 ff.

[6] C. R. Pendleton, The cold receptor, *ibid.*, 40, 1928, 353 ff.

[7] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. königs. sächs Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[8] H. Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biol.*, 83, 1925, 201-206; Die Topographie des Kältesinnes in der Mundhöhle, *ibid.*, 515-534. H. Strughold and M. Karge, Die Topographie des Kaltesinnes auf Cornea und Conjunctiva, *ibid.*, 83, 1925, 189-200; Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges, *ibid.*, 207-212; Vitale Färbung des Auges und experimentelle Untersuchung der gefärbten Nervenelemente, *ibid.*, 297-308.